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ORIGINAL LECTURES.

THE TREATMENT OF WHOOPING-COUGH.

A Clinical Lecture delivered at the Philadelphia Hospital, December 3, 1884.

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(Reported by William H. Morrison, M.D.)

GENTLEMEN: I shall to-day say a few words in reference to the treatment of whooping-cough. Special attention has been attracted to the therapeutics of this disease since the discovery by Burger of a peculiar granular matter in the sputum of whooping-cough, which has been termed the bacillus pertussis. This announcement has excited renewed interest in the treatment of this affection, for, if it be proven, it will alter almost completely the ordinary treatment of this disorder.

Dr. Jenkins, the resident physician, has prepared for your inspection some slides which are now under the microscope. The sputum was placed on the glass and dried over the flame of a Bunsen burner in order to coagulate the albumen. It was then stained with methyl-violet, thoroughly washed, and mounted.

You will note the irregular biscuit-shaped corpuscular bodies. These are supposed to be the cause of the disease, but as yet they have not been successfully inoculated.

It is hardly necessary to enter into a detailed account of the history of whooping-cough. Suffice it to say that it has three stages: the first being that of an ordinary catarrh. In the second stage there is abundant secretion of mucus which is freer than in any other affection. This interferes greatly with respiration, and has much to do in exciting the cough. In the third stage drying takes place, and the paroxysms diminish in number, but not in intensity. Finally the nervous element alone remains, and this gradually dies away. When not interfered with by treatment, it lasts about three months. Among the Japanese, the name applied to this affection signifies "the hundred days cough." At the present time there are methods of treatment by which its course can be materially shortened.

Whooping-cough is subject to many serious complications, especially complications resulting from the extension of the inflammation downwards into the air-passages. It is nearly always accompanied with bronchitis, which may result in pneumonia of the catarrhal variety. There is another serious complication which is due to the excessive expiratory efforts in coughing with obstruction of the larynx from mucus, and this is an emphysematous condition of the lung. If forcible expiration occurs while the larynx is occluded, dilatation of the air-cells necessarily takes place, and if this distention is repeated, rupture of the partitions of the air vesicles occurs, and emphysema results. The older the child, the less is the danger of permanent trouble. In addition to these there are certain cerebral complications which are liable to occur. There is intense venous congestion and even rupture of capillaries with effusion of blood. Clotting sometimes takes place in the cerebral sinus and jugular veins.

If whooping-cough be analyzed, it is found to present two elements: one is the catarrhal element, which, of course, affects the mucous membrane; the other is the nervous element and is very distinct, and it is this that gives rise to the paroxysms. These paroxysms, like the attacks of epilepsy, tend to their own cure. During the paroxysm the blood is almost entirely on the venous side of the circulation, and the anæsthetic effect of the carbonized blood on the cerebral centres relieves the attack.

The latest authorities believe that whooping-cough is a specific disease due to the presence of a microscopic parasite, entering by means of the mucous membrane of the air-passages. Some of the facts pointing in this direction are that it is one of the most contagious diseases of childhood; it runs a definite course, it occurs at all times of life, but one attack almost certainly protects from another. Why this is the case we are no more able to explain than we are in the case of the exanthemata, to which class it seems to belong. It is supposed by some that pertussis has a hardening effect on the mucous membrane of the larynx, which prevents the microbes of the disease from being absorbed. Whether the microscopical germs which have been described are or are not the cause of whooping-cough, is a question to be decided in the future. We know that the affection is due to something which can be carried from one to another. The question whether the symptoms of whooping-cough are due to the action of this peculiar material on the surface of the larynx, or whether it has first to be absorbed and exert its influence through the nervous centres cannot be positively answered. Personally, I am of the opinion that the poison may first get on the mucous membrane of the larynx, be taken up, and act on the cerebral centres. I believe this from several cases which have been reported, one of which was seen by myself, in which whooping-cough has been contracted *in utero*. Nevertheless, careful examinations of the blood, which have been especially conducted by Dolan, who has written an extremely interesting monograph upon this subject, have failed to reveal anything looking like the germ.

The child which I have brought before you to-day affords a marked example of this disease. Yesterday, he had twenty-four severe paroxysms. I call your attention to the condition of the eyes, which show the effect of the marked strain. The eyes are watery, the surrounding cellular tissue infiltrated, and they have a dark ring about them. This is one of the characteristics of whooping-cough. Where it is as marked as in the present instance, the diagnosis can almost be made

from it alone. There are other points in the appearance of those with whooping-cough which are often referred to. One of these is the ulcer under the tongue. The presence of this ulcer has been denied at times while at other times its presence has been acknowledged. The ulcer appears under the tongue on either side of the frænum, and is usually found at the height of the attack. It is due to the forcible expulsion of the tongue which comes in contact with the lower incisors. It is, however, said to have been found in cases in which the teeth have not protruded. The examinations made in this house show that the ulcer is sometimes present and sometimes absent. Where it is found, its presence constitutes a valuable evidence of whooping-cough, but if absent, the idea of whooping-cough is not excluded. At one time the presence of sugar in the urine was considered almost conclusive evidence of the presence of whooping-cough. It was supposed to be due to irritation of the respiratory centre, which we know will produce glycosuria. Examination of the urine of children suffering with whooping-cough reveals the presence of sugar in but few of the cases.

The only real evidences of whooping-cough are the history of exposure to contagion and the presence of the whoop, although this latter is not necessary, for whooping-cough may exist with simply a violent spasmodic cough, but present no characteristic whoop.

As regards treatment, let me say, in the first place, that if let alone, mild whooping-cough usually ends in recovery, unless one of the complications supervene. Previous to the last five or six years, the treatment has consisted almost entirely in the use of antispasmodics. It was considered that the disease chiefly affected the nervous system, and therefore such remedies as asafœtida and belladonna were employed, associated with external counter-irritation. Recent investigations have shown, as I have already stated, the presence of the supposed whooping-cough germ, and this has led to the treatment with antiseptics which has proved so efficacious that I propose to speak of it.

It has long been the custom to send children suffering with whooping-cough to the vicinity of gas-works, and this has been of service. The relief can probably be attributed to the presence of tar; the use of carbolic acid now does this.

We shall begin the study of the treatment of this affection by considering the influence of oxygen. In Germany, the inhalation of oxygen has been recommended especially by Niemeyer, and is said to be valuable. Here we use oxygen in the form of fresh air. Strange to say, that although fresh air is considered so beneficial in this disease, I find, in looking over the mortality tables, that the death-rate in the Southern States is as great, if not greater, than it is in the Northern States. This is probably due to the fact that the large mortality occurs in rickety and cachectic children, and that the children are often exposed to inclement weather, inducing complications. At the same time, the good effect of the soft balmy air of the sea, air saturated with moisture and the chlorides, is undoubted. The paroxysms are diminished in number and intensity, and the complications are warded off.

The next agent to which I shall refer is the steam spray. In some hospitals an attachment is made to the steam heaters, so that a room can be filled with steam. In private practice, steam may be obtained by slaking lime in the apartment, or, more conveniently, by the use of the steam atomizer. If the child remains in the city, he should have the range of several rooms well ventilated and kept at a uniform temperature. The air should be moistened with steam from a kettle on the stove (and each room should have this heat). The atomizer is also useful for the application of medicated solutions to the larynx. There are several of these solutions which may be employed. Carbolic acid is an extremely valuable one. The good effect of the carbolic acid is probably due to the direct action of the remedy on the mucous membrane of the larynx. It is an analgesic and is also supposed to act on the germ of the disease. The probability is that the local anæsthetic effect is the one which is obtained. Dobell's solution is perhaps the best means of applying this drug. It may be prepared according to the following formula:

℞.—Acidi carbolici (cryst.), . . . grs. iij.
Sodii biboratis,
Sodii bicarb., ... āā grs. xx.
Glycerinæ, ... ʒj.
Aquæ, ... ʒv.—Solve.

This can be used with the spray, and diminishes the tenacity of the mucus, favors its expulsion, and exerts a beneficial action on the paroxysms.

Another remedy which has been used with good effect in this way is the oil of eucalyptus, or eucalyptol. Quinine may also be used in the same way, but its bitter taste renders it objectionable for use with the spray, and it is better given in syrup or in the form of a powder. Thymol is certainly a valuable agent in this affection. It is, as is well known, a powerful antiseptic, second only to corrosive sublimate, and is supposed to act directly on the germs. The following prescription can be used with the atomizer:

℞.—Thymol, ... grs. xv.
Alcoholis, ... ʒiij.
Glycerinæ, ... ʒss.
Aquæ, ... ʒxxxiv.—Solve.

Corrosive sublimate, which is now used for almost everything, has also been applied here in the form of the spray. This is a dangerous drug to put into the hands of an unexperienced person, and as we have so many other useful remedies for this affection, I think it wise to avoid the use of corrosive sublimate. Listerine I have used extensively with good results in the treatment of whooping-cough. I employ it in the strength of one drachm to two ounces of water, with an ordinary hand atomizer. I direct the nurse to apply it a dozen or more times a day, and I find that little children, even babies, do not object to it. To it I add tincture of belladonna, potassium carbonate, or ammonium bromide, as the case may demand. The muriate of ammonia is also of great service in the form of the spray. Probably if it were not for the great expense of the drug, the hydrochlorate of cocaine would be valuable in these cases. I have used a spray of the fluid extract of coca with, I think, good results.

When the paroxysms of whooping-cough become very violent, the inhalation of a few drops of ether or chloroform is to be recommended. A few drops of

chloroform may be placed on the hand or on a handkerchief, and the child allowed to inhale it. It is safer to use ether, which is equally efficient. If at the same time the child is turned on its stomach, the mucus will escape by the mouth and the paroxysm will be relieved.

I shall next speak of the use of emetics. The severe paroxysms of this affection are usually brought about by the accumulation of thick, tenacious mucus in the larynx, and emetics act by bringing this away. The emetics ordinarily used in this affection are ipecac, alum, and the sulphate of copper, the latter being recommended very highly by Trousseau. Alum is probably the best. It acts in several ways. In the first place, it produces emesis with the expulsion of the mucus in the throat. It also acts directly on the mucus, coagulating it into small pieces, and facilitates its expulsion. This is considered as valuable as its emetic effect. If it is desired to produce vomiting, it should be given in doses of a fourth or half a teaspoonful, with syrup or honey, and this should be repeated until emesis is produced. The action of the emetic should be facilitated by placing the child on its stomach, with the head lowered, so as to invite the arterial flow to the head and relieve the paroxysm.

We next have to speak of the use of those sedatives which act primarily on the nervous system. It is stated by certain authorities that the disease can be cut short by such remedies as belladonna, bromide of potassium, and sulphate of quinia. I would place belladonna first of all. This has been used for a long time in the treatment of whooping-cough. It must be borne in mind while using this drug that children stand a proportionately larger dose than adults. It has usually been given in the form of the tincture of belladonna, but recently it has been recommended to use the sulphate of atropia in small doses frequently repeated. Belladonna has two effects. It relieves the spasmodic cough, and acts on the mucous membrane of the larynx, drying its surface. It has also been recommended to give atropia by hypodermic injection. The beginning dose of atropia should be the $\frac{1}{200}$th of a grain, and increased until dilatation of the pupil is produced, watching carefully the effects.

After belladonna come the bromides, which are quite useful. These also act in two ways. They exert a local effect on the mucous membrane of the larynx, and also have a sedative action on the nervous system. The bromides of ammonium, of potassium, and of sodium are those most frequently used. The bromide of ammonium acts more directly on the mucous membrane of the larynx as an analgesic, and it is said to act more efficiently when given by inhalation. The bromide of potassium has more of an antispasmodic effect. They can be used together.

Chloral is also very valuable in these cases, and it may be given at night in order to enable the child to rest, or it may be given in a smaller dose several times a day. For a child one year old, the dose would be two grains at bedtime, or one grain repeated three times a day. In my hands this has proven itself a most valuable remedy. Some are afraid to use it on account of its supposed depressing action on the heart. In children, by giving a smaller dose, a good effect can be obtained without danger. My own opinion is, that it is only in old cases, in which the heart has suffered strain, that any dangerous effects need be apprehended from this remedy.

Hydrobromic acid has also been found of service, for it acts like the bromides, and has the effect of drying up secretion. It is frequently used for this purpose in old cases of chronic bronchitis and bronchorrhœa. The dilute hydrobromic acid may be given in ten or fifteen drop doses every two or three hours. Or it may be given in one large dose at bedtime to enable the child to rest. Minute doses of morphia have also been used to relieve the paroxysms, but this I rather object to.

Some time ago, Binz, of Bonn, came out in an article, which was afterward endorsed by Dawson, of New York, recommending quinia most highly in the treatment of whooping-cough. The results of our experiments in this house have shown the efficiency of this drug in many cases. It has been found by experiment that the quinine must come directly in contact with the mucous membrane of the larynx. It has been applied by means of a powder containing the muriate or sulphate of quinia with bicarbonate of sodium and powdered acacia, which is blown into the larynx through a quill. My own plan is to give it suspended in the syrup of yerba santa, which is one of the best vehicles for disguising the taste of quinine. It is necessary to use a less soluble salt of quinine; for if the bisulphate is employed, an extremely bitter mixture will be the result.

The following formula may be used:

℞.—Quiniæ sulph., ... grs. viij.
Syr. yerbæ santæ,
Aquæ, ... āā ℥ss.
Sig.—One drachm represents one grain.

When given for this disease, the quinine must be pushed to large doses, giving it frequently during the day.

In addition to administering drugs to act directly on the throat, large doses of alkali, as the carbonate of potassium, may be given, with the object of liquefying the mucus and promoting expectoration. Where the amount of mucus is excessive, the bronchial tubes choked up, and the child has great difficulty of breathing, especially if there is a little fever and auscultation reveals all sorts of râles throughout the chest, it has been recommended that small doses of tartar emetic should be given. One-fourth of a grain of tartar emetic may be dissolved in four ounces of water, and a teaspoonful given every hour until the difficulty is relieved. It is often astonishing how soon this will afford relief. A few drops of compound syrup of squills which contains tartar emetic may also be given with the same object.

Astringents have also been found to be of service in this disease. For this purpose, alum may be employed, which not only has the effect already mentioned, but also acts as an astringent. Watson, many years ago, recommended that the throat should be painted with a solution of nitrate of silver, but the application is difficult.

Murrel, of London, has used drosera rotundifolia. A few drops of the tincture are put in a glass of water, and used during the day.

Albrecht has used muriate of pilocarpine, which promotes free expectoration and diminishes the tenacity of the mucus.

Benzoate of sodium has also been employed in doses of two or three grains three or four times a day.

Salicylic acid has been found of service by some. Its beneficial effect is attributed to its action on the mucous membrane.

In Germany, sulphur, rubbed up with a little syrup, is used extensively, and, in fact, in some parts of Germany this is the only treatment employed.

Among other things which have been used, may be mentioned tincture of cantharides, in very small and frequently repeated doses well diluted; infusion of chestnut leaves, in the proportion of one ounce of the fresh leaves to a pint of water, and the patient allowed to drink freely of it; also ergot.

The picrate of ammonium is another substance which is supposed to act on the germs. It has been given in the dose of one-twentieth of a grain four or five times a day.

In Europe, the use of calomel has been revived. Small doses of calomel with bicarbonate of sodium and sugar of milk are placed on the tongue. It is supposed to act directly on the mucous membrane while being swallowed.

Lastly, I speak of counter-irritation. This is an important measure. A well-known application consists of croton oil, oil of amber, and oil of cloves. These oils may be mixed with soap liniment or sweet oil and rubbed on the neck or chest three times a day, afterwards covering the surface with oiled silk.

There are so many remedies recommended in the treatment of this disease that we are sometimes at a loss to know how to begin the treatment. In an ordinary case, the use of the spray is of great importance, and no case should be treated without it. With the spray may be applied some of the remedies already referred to, using them in succession until one is found which has a good effect. Small doses of belladonna with alum and bicarbonate of potash may be given during the day to act on the mucus and relieve the paroxysms, and at night a dose of some sedative should be administered to promote sleep. Frequently an enema of ℥j of lac asafœtida will be sufficient. To this should be added the use of a counter-irritant as already recommended.

You should see that the child's bowels are regularly open, either by incorporating fruit with the diet, such as stewed apples or pears or prunes, or an orange before breakfast. Possibly it may be necessary occasionally to give some laxative, such as Philip's milk of magnesia, the syrup of Rhamus purshiana, or once in a while a dose of castor oil.

If the bronchitis be at all severe, the lungs should be carefully watched, and flaxseed meal poultices or flannels wrung out of hot water and covered with oiled silk be applied. In milder cases, a cotton or woollen jacket (carded wool) is to be preferred. When possible, cod-liver oil should be administered, or it may be used in liniment.

As to diet, the most nourishing and easily digested food taken is of course the best. The vomiting may be severe, owing to the constant gagging from the cough. Peptonized milk or gruels, beef peptones, meat jellies, broths, and eggs are the most useful. Milk punch should not be omitted.

ORIGINAL ARTICLES.

REPORT OF A CASE OF SO-CALLED HAY FEVER, TREATED BY THE GALVANO-CAUTERY.[1]

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UNTIL a comparatively recent date, the treatment of hay asthma has been conducted either upon the plan of using palliative measures in the form of local applications, or of internal treatment; or upon the theory of banishing the patient to some place, such as a mountain top or barren island, where there would be freedom from the exciting cause, there to remain till the dreaded season should be over. We believe that most observers admit the exciting cause of this distressing complaint to be some external irritant, such as the pollen of grasses,[2] which, coming into contact with the sensitive and erectile tissue of the nose, sets up an amount of irritation and inflammation there sufficient for the other phenomena of the disease, which are, in fact, only reflex phenomena. That it is not purely a nervous disease, is shown by the rapid subsidence of all symptoms soon after the removal of the patient from the influence of the exciting cause, and by their return, when the patient is even unconsciously exposed to its action. As a good example of this, I remember the case of a lady, who, having gone to a seaside place specially recommended on account of its barrenness, was much surprised to find herself suffering so much from her trouble during the first night. On arising in the morning and looking out, she saw there was a garden by the house, of which, as she had arrived after dark, she had been entirely unaware.

Although removal from the cause gives positive relief, most can ill-afford such prolonged banishment from the business of life; and when, as in the case which I am about to relate, the affection lasts from the beginning of June to the end of August with great violence, the question as to what is to be done becomes very serious.

I do not consider it within the scope of this paper to speak of the various local and general remedies that have been tried and recommended. Suffice it to say, that most of them afford temporary relief to some, while to others they are without influence. A few seem to be greatly relieved by them. In general, I have noticed that chronic sufferers are slow to believe in any cure other than the relief afforded by temporary exile.

But the external excitant is, of course, only one link in the chain of causes that produce the disorder. A very important link is formed by the chronic disease in the nose which is always found in those

[1] Read before the Clinical Society of Maryland, Jan. 16, 1885.

[2] The pollen of grasses is, of course, only one of many excitants. I have now under my care a lady who is suffering from this disease, who is never so free from it as during the summer. In her case, the attacks are brought on by dust, bad odors of any kind, etc. In the case of a colored man, who lately presented himself at my clinic, attacks were brought on whenever he entered a room where oysters were being opened.

affected with hay fever. It is very possible that the patients themselves may not be aware of the existence of any trouble of the kind between the attacks. They will say: "I am quite well, except during the season." This is probably owing to the fact that, compared with their sensations during the acute affection, they feel well for the rest of the year. Nevertheless, examination by the rhinoscope reveals the presence of diseased tissue, and it has been found that when certain parts in the nose, sometimes over the turbinated bones, sometimes on the septum, are touched with a probe at any time during the year, all the symptoms of hay fever will momentarily return. The most frequent condition found is hypertrophied tissue over the turbinated bones, especially the inferior, and over the septum. This may be comparatively slight, or so great that breathing is interfered with through the nose. In addition to this, polypi and other growths are often noticed, or a projecting spicule of bone, or a deflected septum. We can see, therefore, besides the individual susceptibility of certain people to irritation of this kind, the exciting cause needs an abnormal condition of the nasal passages to enable it to produce an attack of so-called hay fever. If, then, we remove this unhealthy condition, the chain of cause and effect in the production of the disease is as surely broken by the breaking of this link as by sending the patient to some barren island.

With Dr. Roe, of Rochester, N. Y.,[1] and others, I fully believe that this method of removing the susceptibility is the most satisfactory and positive method of cure. In fact, it is the only method of cure, as all other procedures are only temporizings. It consists in local treatment, and small operative procedures. Of course, everything calculated to excite irritation must be removed. If the septum be markedly deflected, it must be straightened; a projecting spicule of bone must be removed, as well as any polypi or other morbid growths. The hypertrophy which is nearly always present, if large enough, should be removed by a Jarvis's or other snare. If the snare is not applicable, caustics should be used, of which the galvano-cautery is, in my opinion, decidedly the best. Besides this, any point of irritation, as discovered by the probe, should be touched by the galvano-cautery, using only a very small tip. In some cases, as those described by Dr. Roe, in which there seems to be no hypertrophy, and the only sign of disorder is the irritation caused by the probe, this last is all that is required.

As to the time of treatment: for the cure, the best time is between the attacks, as there is then no risk of increasing the irritation. After the main treatment is over, it is very desirable for the patient to be seen during the next hay fever season, so as to touch any small remaining areas of irritation with the galvano-cautery tip, and render the cure radical. The following case is of special interest, owing to its unusual severity, the attacks being so severe as almost to endanger life.

B., an English gentleman, aged 22, unmarried. Since early childhood he has been subject to a certain amount of nasal catarrh, and for a long time—he cannot fix the date, has been unable to breathe for more than a minute or two at a time through his nose. About six years ago, he noticed an increase in his catarrh during the summer. Every year it has grown worse, commencing about the 1st of June and continuing till the last week in August. The attacks assume the form of asthma; and when they are at their height, he is unable to sleep or to lie down. Cannot walk across the room without increasing the dyspnœa. Many remedies have been tried, with only temporary relief, and last year the attack, after lasting a long time, culminated in catarrhal pneumonia, which left him so weak and delicate that he was unable to return to his business till November.

This summer (1883) the asthma returned with increased violence, and his physicians, fearful of a repetition of the experience of the previous year, recommended a sea-voyage. He landed in New York, July 18, 1883. This was just as I was starting on my summer vacation, and so, having been informed of his coming, I met him at the wharf. After the second day of the voyage, he had been entirely relieved of his asthma; but so sensitive was he that he had not been more than three hours in the city before he began to suffer from dyspnœa, and the usual accompaniments of the disorder. That evening, I took him by boat to the Island of Nantucket, knowing that there was a special place on the island, called Wauwinet, where he would probably be free from his trouble. There was very little vegetation near the little hotel, and, as the strip of land between the harbor and the sea was only a few hundred feet across, every breeze was by necessity a sea-breeze. Here he was quite relieved, except when a bunch of flowers was brought near him, when he would at once begin to feel suffocated. The disease, however, though it gave him no trouble, was only held in abeyance, as I had opportunity to observe by the rhinoscope, the tissues of the nose being in a state of very active hyperæmia.

Early in September, he came to Baltimore for treatment, but the extreme irritation of the Schneiderian membrane rendered it inadvisable to commence active local treatment at once. I therefore ordered full doses of quinine and had him evaporate "vapo cresoline" (a preparation of coal-tar) in his room at night, at first for an hour or so before retiring, and, later, through the whole night. This proved very satisfactory in lessening the irritation. The condition of the parts at this time was as follows, viz.: Very marked hypertrophy over the middle and lower turbinated bones anteriorly and posteriorly, not, however, projecting into the naso-pharynx; adenoid growths in the vault of the pharynx; follicular pharyngitis, and also chronic laryngitis. These last were evidently secondary to the naso-pharyngeal disease. Repeated examination of the heart and lungs showed both to be healthy. On October 4th, after some local treatment, consisting of applications of dilute spts. vini rect. to the nose and iodinized glycerine or solutions of argent. nit. (gr. xl–lx ad ℥j) to the pharynx, I made the first application of the galvano-cautery to the tissues over the right inferior

[1] New York Medical Journal, May 12 and 19, 1883, and May 3 and 10, 1884.

turbinated bone anteriorly.[1] Subsequent applications of the same kind were made to the hypertrophied tissues anteriorly and posteriorly. The posterior applications were made through the mouth under the guidance of the rhinoscopic mirror. The snare was not used during the treatment. The enlarged follicles in the pharynx were touched in the same way, and the adenoid growths removed with the forceps. The following precautions were observed in making the galvano-cautery applications, viz.: For a day or so before and after, I administered full doses of tinc. ferri chlor., and never made a second application in the vicinity of the first till all the irritation caused by the first had subsided. He returned home on the 21st of November.

The following are, from my notes of his condition for several weeks before he sailed: He can breathe with perfect ease with closed mouth, and is able to read aloud with a facility before unknown. He has no special susceptibility to cold, and is able to bear exposure to inclement weather and night air. Even if he catches a slight cold, he is able to throw it off with ease. He says he is better than he ever was in his life.

He passed the winter with decidedly improved health and with very much greater freedom from any bronchial trouble. In the spring he exposed himself on several occasions to special risks, and had an attack of subacute bronchitis, which, however, passed off before the time for the hay fever. About the same time, he moved to Liverpool, choosing a room with but little sunlight in it.

I heard from him repeatedly during the dreaded period, and learned that he was hardly troubled at all. On one occasion, at the end of a short visit to one of the most thickly wooded districts in England, he felt a tingling in his eyes and for three days had a slight cold in his head. In August, for a few days he feared he would have a return, but it also passed off. His usual three months of suffering, therefore, were shortened to a few days' comparative discomfort. His was the worst case of this disease which I have seen, as the sequelæ of the last attack absolutely threatened his life, and thus I think it amounted to a test case.

ON THE SURGICAL DISSEMINATION OF CANCER.[2]

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THE subject in question not being mentioned by the authors of text-books on general and special surgery, and unknown in periodical literature, seems of sufficient importance to deserve consideration. The present essay is an *a priori* consideration of the possibility or probability occurring in the course of the development and treatment of cancer; an outline of a train of thought based on clinical observation that may deserve more extensive study in the future.

The subject-matter itself is not strictly new, observations bearing upon it having been made and published from time to time in an unconnected manner. The leading idea of this essay was originated in the year 1877, by observing the course of a striking case of rapid dissemination of cancer. Since then a number of other cases have accumulated, tending to confirm those first impressions, when, finally, Czerny's observation (published in 1883), and König's remarks on operative dissemination of tuberculosis (published in the *Centralblatt für Chirurgie*, 1884), appeared, the interest of the author was quickened by their strong confirmatory, though indirect, evidence.

They both, like the author, did not advance anything absolutely new, as everybody knows that many patients operated on for joint-tuberculosis die subsequently, and soon after the operation, of meningeal, or other forms of disease. But they drew attention to the fact that the *post hoc* may be in many cases also a *propter hoc*.

The author's case in question was as follows: An aged lady, of robust health, and cheerful, intelligent disposition, presented herself for treatment on account of an elevated ulcer of the outer margin of the sole of her right foot. It had existed for over thirteen years, growing very slowly, until it had attained the size of three inches by two inches and a half in diameter. It was segmented, each segment showing a distinct epidermal margin, the edges of the ulcer overlapping somewhat the normal skin. While open, the purplish-red ulcer discharged large quantities of a clear serum, but if the patient refrained from walking it readily cicatrized over, to become sore again on her using the foot. Previously the pain or inconvenience was very slight, and no glands were infiltrated. Lately, however, the sore had become very painful.

Excision was proposed, and carried out in the fall of 1877, when the base of the new growth was found to be attached to the plantar aponeurosis, which accordingly had to be removed too. The wound healed rapidly by granulation, the diagnosis of granuloma made previous to the operation, and based upon the benign development of the disease, was confirmed on microscopical examination made by Dr. I. Adler, of this city. A favorable prognosis was expressed, and the patient was encouraged to use her foot. Immediately upon cicatrization being completed, a number of small, purplish, pigmented nodules made their appearance in and about the scar, and well up the leg, increasing daily in number and size. Any doubts about the formidable character of the new growth were dispelled then, and a high amputation of the thigh was at once proposed, but declined by the patient, who, however, finally submitted to the operation in May, 1878. A mass of enlarged glands was also removed from below Poupart's ligament. Both wounds healed by the first intention, but the operation did not delay the patient's impending fate. A few days after the removal of the limb the patient inquired about the significance of a disturbance of vision, which she had noticed for four weeks, but in her excitement preceding the amputation omitted to mention it to the author. It will suffice to say that a retrobulbar node, causing first diplopia, finally pro-

[1] The instrument used was the improved Seiler cautery instrument.

[2] A paper read before the New York Surgical Society, February 10, 1885.

truded the eyeball from its socket in a frightful manner, and that the entire body of the patient became covered with innumerable nodes and nodules in an astonishingly short time. The patient's death by suicide fitly closed the tragedy, which did not fail to make upon the author a deep and lasting impression. Post-mortem examination revealed that all the organs, especially the viscera, were permeated with thousands of nodules of pigmented cancer.

Undoubtedly, then, we must have had originally a true granuloma, which at the time of the first operation was undergoing the not unusual change into cancer; some portions of the tumor then removed still presenting the character of a connective-tissue growth, as Dr. Adler's examination had proved. But is the fact of this change of a benign into a malignant neoplasm alone sufficient to explain its astonishingly swift dissemination? Certainly not, as all processes of dissemination observed in malignant new growths, local and systemic, are notoriously slow, and not to be compared with that in the foregoing case. By "slow" is understood a spread in *étapes*, as the French say, on marches, to use a strategetical term. That is, a nidus which is not continuous, but interrupted, is characterized by well-defined phases of activity and seeming rest.

First comes the simplest and most immediate spread by contact with neighboring tissues, which generally lasts for a considerable time, but soon is followed by the second phase; that is, that of the infection of the adjacent lymphatic vessels and glands. These serve as a barrier for a certain period of time, till a large vein becoming involved, transplantation by embolism extends the disease to one or more rather distant organs. This last one would form the *third* or *generalizing* phase of infection.

In the case related, local and general dissemination seems to have taken place simultaneously, and in a manner closely resembling certain physiological and pathological experiments performed on animals, where substances capable of multiplication are injected into the parenchyma, or directly into the circulation. The assumption of a mechanical, rapid distribution of the infective bodies is almost imperative, and this rapid mechanical distribution of the infective elements may have been accomplished by the manipulations incumbent upon the performance of the operation.

Observations similar to, if not as poignant as the one related, have multiplied in the experience of the author, extending over forty cases of amputation of the breast made since the year 1878, and the practice of every busy surgeon must yield a not inconsiderable number of instances of the same character. As none of the cases of this character possesses the elements of proof positive, it would be unnecessary to weary your patience by records of similar cases, but it is important to tell you that the practical wisdom of an older generation of surgeons has impliedly admitted the truth of our assumption by advising amputation of an extremity afflicted by what was then termed "cancer," in preference to a mere extirpation of notoriously malignant growths. And the value of this advice is twofold: First, it more surely eliminates all foci of disease than a however comprehensive excision; and last, but not least, the field of operation being more or less distant from the seat of the disease, handling of the tumor and manipulative dissemination of its elements are less likely to occur than otherwise.

Now let us consider what amount of force and energy is employed in the handling of tumors by physicians, and whether this handling does not possess some or all the characters of that form of manipulation which the French denote as "massage."

A careful physical examination of a tumor, especially if made by the aid of anæsthesia, will be, and notoriously is, frequently accompanied by the use of a good deal of necessary or unnecessary force. The handlings or manipulations ordinarily in use during the performance of bloody operations on tumors, to be dissected out of surrounding tissues, have positively the character of massage, and occasionally a very rough form of massage, too. And the employment of anæsthetics has certainly not had the effect of increasing the gentleness of operative interference. Suffice it to say that the manipulation employed on a tumor of the breast and the lymphatic glands occupying the adjacent axillary cavity during an operation lasting one or two hours, certainly may have the dignity of a manipulative séance. And all of you know that a skilful massage of thirty minutes will often cause to disappear a massive extravasation of blood from the vicinity of a sprained joint, and that three or four sittings may dispel the largest part of a solid infiltration of an inflammatory character from this or that part of the body. Now just bear in mind how many formed corpuscular elements, be they cells or broken-up portions of a firm clot, must be propelled through the lymphatics and veins into the general circulation at each massage, and the idea of the probability of the propulsion of cancerous elements of an unfixed, minute, that is, embryonic character, and situated especially in recently involved places about the margins of a tumor, is very plausible indeed. The analogy of these artificial processes is *a priori* evident, and the fact that mechanical irritation, that is, another form of massage, favors the spread of malignant disease, stands fully in accord with this assumption. Finally, it seems appropriate to adduce here Billroth's authority as a pathologist in these, his words: "I consider it most highly probable that both the continuous and the discontinuous spread of carcinomata is effected (*vermittelt*) by the dissemination of corpuscular elements, but admit that so far we are unable to prove this in all cases.[1]

The author does not wish to have it understood as if he considered this mode of dissemination the rule, but he believes that in a number of cases of malignant disease operative treatment may hasten, rather than retard, death. This statement seems to contradict the results of statistical investigation as laid down by such authorities as Von Winiwarter and Gross, whose meritorious works are well known to all surgeons. But this contradiction is partial only, and is justified by the nature of statistical results in general, which we know are arrived at by summing together

[1] Deutsche Chirurgie; Krankheiten den Brustdrüsen, S. 112.

cases widely different in character, though all representing the same disease.

Our critical attention will necessarily be first directed to the comparative estimates by averages of the duration of life of patients operated on for cancer of the breast, and patients not subjected to operative treatment.

Here we see that Winiwarter finds the average duration of life from the beginning of the disease of the operated cases to be about 39.5 months, whereas that of patients left to their fate is almost 33 months. The operated cases, accordingly, live on an average about six and a half months longer than those not coming under the surgeon's knife.

On further examining the figures it will be remarked at once that the number of observations of non-operated cases is very small as justly compared with those operated on, one being 97, the other 519. Take into consideration, furthermore, that many, if not all, of the non-operated cases were subjected to all manner of irritative treatment, greatly influencing their rapid development and dissemination. Finally, that many of them may and have been such as show a formidably malignant character, deterring even the boldest operator, so that it will be easy to comprehend that the rate of mortality as calculated for these 97 cases is unreliable and too high if applied to all cases of unfavorable cancer, many of which not being malignant did not come under observation at all. But, let us analyze the cases subjected to operation. Here we find that somewhat over *nine per cent.* were permanently cured by the operation, They necessarily must have been such in which a complete removal of all diseased tissues was successfully accomplished. It is of the utmost importance to note that such a thing is possible under certain favorable conditions, offered by an early and very thorough operation of a not extensive tumor of comparatively slow growth. Clinicians know and admit differences of infection in cancers which depend upon a more or less rapid growth, and accordingly upon a greater or less proportion of succulency attributable to more or less embryonic, non-fixed or movable tissue components or corpuscular elements. Their anatomical expression is a greater or less proportion of *cancer juice*, as produced by scraping of their cut surfaces with a scalpel.

To go back to a former comparison, we may say that as the corpuscular or liquid elements of a recent succulent inflammatory infiltration are more easily dispersed by massage, so will the elements of a succulent cancer be more prone to be dispersed by mechanical factors, and we may add that their rapidity of reproduction also stands in direct proportion to the ease with which they are dispersed. This relationship suggests resemblance to a geometrical progression, and may serve to explain the enormous differences of malignity observed in cases standing at the foot and at the head of the statistical scale respectively. Gross mentions an unoperated case that terminated fatally at the expiration of five months. Billroth gives the history of one that lived for twenty years.

To return to the cases cured permanently, we may say that a few of them may have been very malignant, but that most of them were not very malignant and were subjected to a very radical operation: finally, that in all of them up to the patient's death from other diseases, no formation of cancer *de novo* had taken place. (It is still a mooted question whether even a regional recurrence of cancer three or more years after operation is a continuation of the old process, or a new disease *per se.*)

The large majority of the 368 operated cases, that is, nearly 81 per cent., showed recurrence. In eleven and a half per cent. of the 368 cases, recurrence took place after twelve months; but it does not appear in what manner or where. It would be important to find whether the general character of these recurrent tumors was mild or severe, local or general, and what was their microscopical nature (succulent, or firm and dry).

Presumably these were mostly tumors of not great malignancy, or of a slow growth, incompletely removed, yet so that very few cancerous elements were left behind to multiply.

It may be pertinent here to mention the curious fact, though no explanation of it is ventured, that seemingly infected glands have been seen to disappear after the removal of the primary tumor in such cases even where the knotty, hard nature of the glandular swelling fully justified the assumption of its cancerous character.

Within three months sixty-three per cent. of the cases relapsed, forty-three per cent. within *four weeks*, and nineteen per cent, even before the lapse of a *fortnight.*

In about 82 per cent. of 194 cases of recurrent cancer in which this point was noted, the locality of the relapse was in the cicatrix, remains of mamma or vicinity, and in the glands.

These figures are appalling; they show that in almost two-thirds of all operated cases relapses are established within three months after the operation. Undoubtedly the great majority of these cases represent very incomplete operations, where already disseminated portions of the neoplasm were left behind, either in the vicinity of the primary growth, or in or about the infected lymphatic vessels and glands. The cases relapsing within four weeks (43 per cent.), and still more, those recurring within a fortnight (19 per cent.), must be such where either incomplete operations were performed, or where artificial operative dissemination took place, or finally where both of these influences combined to cause a rapid involvement of hitherto seemingly healthy tissues. We have to add, finally, while treating of this group, a class of cases characterized by Billroth in the following words: "Cases operated on under seemingly very favorable circumstances, in which, however, the patients are already victims of internal carcinoma, this becoming evident on post-mortem examination made soon after the operation, are unfortunately *not very rare.*"[1]

Of course, the occurrence of such cases cannot be gainsaid, but considering the gradual extension of cancer observed as a rule, such an infection by a jump, as it were, must nearly always be caused by

[1] Loc. cit., p. 117.

mechanical factors. By these are meant either a build and arrangement of the lymphatics usually favorable for the easy transmission of corpuscular elements, or a very energetic and effective external agency, such as a traumatism, or massage, or long-continued compression, or all of these combined.

But to return to our subject, we will say that Von Winiwarter does not state whether a change in the character of the malignancy of these neoplasms was observed or not on their recurrence after operation. And herein all observations are strangely defective, and unfortunately so, as this seems to constitute the nodal point of our inquiry. If it were shown that in a certain number of tumors of previously slow growth, a fast and extensive *local* recurrence took place immediately after a comprehensive and thorough-going operation, performed by a competent and experienced surgeon, it would be necessary to assume the concurrence of a factor hitherto unknown, even unthought of. And if the marginal portions of the removed tissues were found, on careful and exhaustive examination, to be entirely free of such miliary foci of cancer as can be detected by the microscope, the presumption of a direct influence upon the nature and course of the disease, by the operation, would be almost imperative.

Older surgical writers directly admit the possibility of such an influence; modern authors, as, for instance, Billroth and Volkmann, only by inference; and their reticence in this respect is easily explained by the lack of a sufficiently large sum of observations collected with a view to the ascertainment of this very point: and by the necessity of questioning the correctness of the generally accepted assumption that the operated cases show a much longer average duration of life than those let alone.

Let us now examine what this peculiar "influence" of operative or other mechanical irritation upon a cancerous growth may consist in. Excluding chemical irritation, and taking in view only forms of irritation considered to be more strictly mechanical and akin to massage, we find that this influence may be twofold. On the one hand, it may induce active hyperæmia, a better nutrition of the tumor, and hence a more rapid multiplication of existing elements *in situ*. On the other hand, and following from the first, it will increase the activity of the lymphatics, and fitly prepare them for the reception and transmission of mobile cancerous elements. Finally, the changing degrees of pressure from different sides, and its intermittent character, will not fail, in suitable cases, to propel such cancerous elements to a shorter or longer distance, and if the form of mechanical irritation is constant and frequent, a large area of the surrounding tissues may become injected with cancerous elements in a comparatively short time. Supposing that all this takes place during an operation, followed by an unavoidable stimulation of the thus infected tissues incumbent upon the processes of repair, what wonder if we see a large number of small nodes appear in and about the cicatrix, left after the removal of a solitary tumor, and within a few weeks after the operation?

Instead of assuming a mysterious, indefinable change in the character of the new growth, it is more rational to explain a natural process by natural and well-known biological laws. It may be objected that all operations followed by a rapid and wide-spread reappearance of the disease about the sites operated on, must be such in which preëxisting foci of cancer were unwittingly left behind by the operator. Admitting, as we have done before, that a large number of cases is covered by this explanation, yet we may reply to this with the following question: What is the reason, then, of the simultaneous appearance and rapid growth of a large number of cutaneous foci shortly after the operation on a formerly slowly growing tumor? Why do these foci remain dormant, or grow imperceptibly until the time of the operation, and suddenly come forward after the traumatism of an operation? And there is not only a spread, there is frequently also an increased rapidity of the multiplication of the elements, an increased malignity denoted as a *change of character;* and why this? Our explanation seems to cover these questions.

Besides, it is not impossible that young cancerous cells, sowed, as it were, into new soil, well prepared by an increased blood-supply due to the healing process, will thrive much better in this unexhausted, well-irrigated ground than elsewhere.

Of course, all this does not prejudice the acceptance of the fact that positively benign tumors do change occasionally into malignant ones, but it seems that this important mechanical factor in the growth and dissemination of cancer ought to receive some recognition. The author has begun a series of experiments tending to prove the possibility of mechanical or surgical dissemination. But the results of the investigation of the subject have not matured yet for publication.

Before completing this paper it may be appropriate to point out in a few words its practical bearings. It is not intended to question the general correctness of the advisability of operating on even far-gone cases of cancer. On the contrary, it is hoped that by pointing out one more danger, we may be taught to avoid this also, thereby succeeding more and more to improve the results of the operative measures in this field. Antiseptic treatment has here, too, undoubtedly stimulated active surgical interference, and though the author's material includes only thirty-four radically operated cases of malignant tumors of the breast, that is, such in which the breast and the axillary contents were removed, and partial operations, with no deaths resulting from the operations, this may justify the conclusion that antiseptic measures must have generally depressed the death-rate of 16.5 per cent. as given by Winiwarter.

Nothing need be said about the necessity of early operations, and the very radical, extensive removal of a wide zone of seemingly healthy tissues surrounding a cancroidal tumor. If the neoplasm be on an extremity, and is extensive, the preference should be given to an amputation made far away from the disease. Wölfler has conclusively shown on animals that during the time that Esmarch's constriction is applied to a limb the veins and lymphatics beyond the constricting band are inactive, and do not absorb even soluble substances, and that these do not enter the ostia of the vessels opened up by the sur-

geon's knife as long as the constriction lasts.[1] It seems to be rational, then, always to employ Esmarch's constriction when existing malignant tumors are situated on a limb, since it is improbable that corpuscular elements would be propelled where there is no medium, as blood or lymph, to carry them along. In examining any kind of tumor the utmost gentleness should be observed, and obscure points in the diagnosis not bearing upon the therapy, should rather be left undecided till after the operation. Unnecessary handling of a suspicious tumor should be discouraged, and the patients especially warned against so-called abortive methods of treatment, as painting with irritating chemicals, and especially, the several forms of compression, constant or interrupted.

As a preparatory measure, the hyperæmia and cell-activity of a rapidly growing tumor should be reduced as much as possible by the constant application of ice, or the water coil, for several days preceding the operation, as it is notorious that the growth of some tumors is noticeably retarded by the influence of low temperatures.

During the exsection of deeply embedded tumors all rough handling and pulling to and fro, should be avoided as much as possible. This may be very difficult in cases of large, heavy tumors, or where it is necessary to work in a deep, non-dilatable wound, wherefore the proposal of an English surgeon to fasten into each tumor, after its exposure, a sharp hook attached by rope and pulley to the ceiling of the operating room, seems to be very appropriate. One assistant can easily raise or lower even a very large tumor by traction applied to the rope running through the pulley, while the surgeon himself, holding the shank of the hook, can carry out without fatigue such lateral movements as become necessary in the course of the operation. This would deserve a trial, especially in amputations of the female breast. For self-evident reasons the axillary fat should be invariably removed, and in one mass, this being much easier and safer than the digging out of single glands. The operation should commence, if possible, with the evacuation of the axillary space and its contents, which, together with the breast and the intervening tissues, should be removed in one piece.

Primary union of the entire operation wound should be always aimed at where possible, since suppurative processes, and the increased reproductive activity of the tissues incumbent thereon, tend to cause an early development of such cancerous elements as may have been left behind.

The view that non-destructive suppuration may eliminate such cancerous elements, has no support in pathology. The surgeon should rather use his knife freely than invoke the aid of such a treacherous ally as suppuration.

[1] Archiv für klin. Chirurgie, Band 27, S. 413.

OBSERVATIONS ON NIGHT-COUGH, ESPECIALLY IN YOUNG CHILDREN.

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IT is the common experience of physicians to meet many cases (in young children chiefly) of an obstinate and irritative cough, which is particularly active at night, and which has led practitioners a devious path in the attempt to medicate it successfully. This irritative cough has caused much anxious solicitude to parents, and has brought to many little ones days of lassitude by reason of broken sleep. It appears to me that the *true* nature and cause of many of these irritative, hacking coughs have been overlooked or underestimated; nor can successful management be expected until the pathological factor present is fully appreciated.

The majority of children who have this annoying symptom at night show no appreciable illness during the day, nor is the cough then often present. Close surveillance only will detect an occasional clearing of the throat in older children; in the younger ones, occasional attempts at swallowing. In certain cases escape of mucus from the nostrils may be seen, while others require a critical inspection of the nasal chambers to reveal the presence of discharge. It is often appreciated that the child has a "cold in the head," but that any causative relation exists between the condition of the nasal passages and their products and *the cough* is not imagined.

It has been so long the rule to ignore affections of the nose in adults that it is not to be wondered at that the mucous membrane of the nasal passages in children should receive so little attention. I am convinced that there will be a complete revolution in our ideas of diseases of the nose and naso-pharynx in their *relation* to diseases farther down in the upper air-passages; and that the nasal cavities will become most important factors to be considered in properly appreciating affections of the larynx, bronchi, etc. A considerable experience in this class of cases has led me to formulate the following propositions: 1st, that many coughs are *not* dependent upon any inflammatory or catarrhal processes in larynx or bronchi; 2d, that they are *not* generally dependent upon prior enlargement of the tonsils or pharyngitis; but, 3d, that they *are* dependent solely upon a nasal catarrhal inflammation with its accompanying secretion.

It has been demonstrated by Dr. John N. Mackenzie and others that certain areas of the nasal chambers are quickly responsive to irritants or stimulants, and that mechanical irritation applied to the regions embraced by the turbinated bodies and septum posteriorly, will quickly cause an explosive act *quite* like a cough. I have also demonstrated, to my own satisfaction that the reflex cough area is not at all confined to these regions,[1] but that in a large proportion of cases, the anterior portion of the septum and vault of the nose are highly sensitive to irritants and constitute a remarkable active area of sneezing and cough. I have cases to show that the mucous membrane covering the vault or bridge of the nasal chambers, and a portion reflected upon the septum, is chiefly concerned in the production of hay asthma, serous catarrh, etc. Now, if we bear in mind these interesting facts concerning nasal cough and its production, as well as the more common occurrences of mechanical cough, we can easily accept the doctrine herein advocated.

[1] Confirmed by Hack, Wien. med. Wochenschrift, 1882-83.

The rationale of the attacks of night-cough occurring in children (and adults also), as soon as the recumbent posture has been assumed, is, that while the child is in the erect position during the day, gravity lends its force to *facilitating* the escape of the secretion from the nasal passages, either through the nostrils or posterior nares, assisted by the functional activity of the fauces, palate, larynx, etc. But at night the child retires with an accumulation of more or less discharge in the nasal passages, or some is secreted after a few hours. The child is lying down, and the effect of gravity then is to aid the discharge in gradually escaping from the posterior nares, when it forms an elastic pellicle which gradually lengthens out and slips downward along the pharyngeal wall until the free edge of the epiglottis is touched by it, when instantly the interference is resented—a cough expressing it. Again and again the effort is repeated; a constant hacking cough supervenes from this purely *mechanical* irritation. If the pellicle of mucus passes the epiglottis, it lodges upon the posterior or arytenoid portion of the larynx, and gives rise to the same symptoms. The pellicle of mucus may still be attached high up in the naso-pharynx, and being very tenacious, it can be readily understood how the cough may be kept up for hours before the mucus becomes detached and passes beyond the larynx.

It is a well-known fact that a drink given to the child will often cause a cessation of the paroxysm, the fluid simply pushing downward the exciting foreign body. This clinical fact gives us the clew to the successful management of these troublesome cases. If the nasal passages are thoroughly cleansed before the child is put to bed, the night will probably be uninterrupted by this irritative cough. If the discharge accumulates in the nasal passages or upper pharynx during the night and gives rise to a paroxysm, it is a simple procedure to cleanse the nasal chambers, and at once relieve the symptoms. Treatment can be most effectively carried out by means of a spray composed of an aqueous solution of an alkali. By its use these cases need no longer be harassing to either practitioner or patient.

1417 WALNUT STREET.

MEDICAL PROGRESS.

THE RELATION OF BACTERIA TO ASIATIC CHOLERA.—The investigations of the English Cholera Commission as to the relation of bacteria to Asiatic cholera has, according to DR. KLEIN, ed to the following conclusions:

1. Koch's statement as to the constant occurrence of comma-bacilli in the rice-water stools of cholera patients is correct; the comma-bacilli vary greatly in numbers in different stools and in different cases, in some being exceedingly scarce, in others numerous.

2. These comma-bacilli vary greatly in length, some being twice and three times as long as others, some well curved, as much as to form half a circle, others showing only just a slight bend. The name comma-bacillus is inappropriate; the organism is more correctly termed a vibrio.

3. The comma-bacilli occur in the mucus-flakes of the rice-water stools, as well as in those taken from the ileum of a person dead of cholera. The sooner after death the examination is made, the fewer comma-bacilli are found in the mucus-flakes; even in typical rapidly fatal cases, the mucus-flakes taken from the ileum, and examined soon after death (from between fourteen minutes to an hour or an hour and a half), contain the comma-bacilli only very sparingly indeed, and not to the exclusion of other bacteria. Our investigations do not bear out Koch's statement as to the lower part of the ileum being, in acute typical cases of cholera, almost "a pure cultivation of comma-bacilli." In not one of the many post-mortem examinations of typical acute cases have we found such a state.

4. The mucous membrane of the ileum, in typical, rapidly fatal cases, if examined soon after death, does not contain in any part any trace of a comma-bacillus or any other bacteria, not even in the superficial loosened epithelium. If the post-mortem examination be sufficiently delayed, comma-bacilli and other bacteria may be found penetrating into the spaces of the mucous membrane. Koch's theory as to the comma-bacilli present in the mucous membrane secreting a chemical poison inducing the disease cannot, therefore, be correct.

5. Neither the blood nor any other tissue contains comma-bacilli or any other microörganisms of known character.

6. The behavior of the comma-bacilli in artificial media is not such as to justify their being considered as specific. They grow well in alkaline and neutral media, are not killed by acids, and their mode of growth in gelatine-mixtures is not more peculiar than that of other putrefactive bacteria; they show marked differences when grown in different media, but not more so than the ordinary putrefactive bacteria when compared in their growth with one another. The comma-bacillus of the mouth shows the same peculiar character of growth in gelatine as Koch's comma-bacilli.

7. Koch overlooked the fact that "comma-bacilli" occur in other intestinal diseases, in the mouths of healthy persons, and, as shown recently, even in some common articles of food. (By Dr. Deneke in stale cheese.)

8. The experiments performed by Koch and others on animals do not in the least prove that the comma-bacilli are capable of producing cholera or any other disease. The results obtained by them are much more easily explained in an opposite manner.

9. There is direct evidence to show that water contaminated with choleraic evacuations, and containing, of course, the comma-bacilli, when used for domestic purposes, including drinking, by a large number of persons, did not, in the case of the tanks near the Jelepara Lane, produce cholera.

10. The mucus-flakes taken from the small intestine of a typical, rapidly fatal case of cholera, contain numerous mucus-corpuscles filled with peculiar minute straight bacilli; in this state they are found when the examination is made very soon after death; soon, however, the mucus-corpuscles swell up and disintegrate, and then their bacilli become free. The small bacilli are never mixed in the mucus-flakes. They are one-third or one-fourth the length of the comma-bacilli, and about half

their thickness. They are non-mobile; they grow well in Agar-Agar jelly, but show in their modes of growth no peculiarity by which they could be considered as specific. When grown on the free surface of the nourishing material they form spores.

11. These small bacilli are not present in the blood, in the mucous membrane of the intestine, or in any other tissue.

12. Experiments made with these small bacilli on animals produced no result.

13. Since his return to London, Dr. Klein has ascertained that the comma-bacilli of cholera show two distinct modes of division, one the known one of transverse division, and a second one of division in length. When growing in Agar-Agar jelly at the ordinary temperature of the room, after some days the bacilli swell up, owing to the appearance in their protoplasm of one or more vacuoles; as these vacuoles increase, so the comma-bacilli become gradually changed, first into plano-convex, then into oblong biconvex, and ultimately into circular corpuscles. The longer the original comma-bacillus, the larger the final circle. These circular organisms are mobile, just as are the comma-bacilli; and, by disintegration of the protoplasm at two opposite points, two perfect more or less semicircular comma-bacilli are formed. Growing the comma-bacilli in Agar-Agar jelly kept at higher temperatures (30° to 34° C.), the comma-bacilli multiply by transverse division only; but, transferring these to Agar-Agar jelly, and keeping this at the ordinary temperature of the room, they again gradually change into circular organisms, which, by division in the diameter of the circle, form two new comma-bacilli.—*British Medical Journal*, Feb. 7, 1885.

DIGITAL DILATATION OF THE PYLORUS.—DR. PIETRO LORETA has successfully practised dilatation of the pylorus, for the cure of stenosis, in five cases, from which he concludes:

1. Digital dilatation of the pylorus should be a recognized operation in practical surgery, since it may radically cure stenosis, consecutive to obstacles lying in the pylorus, cicatricial stenosis, displacements of the pylorus, foreign bodies, echinococci, tumors, etc.

2. Cicatricial stenosis and other causes of obstruction to the passage of chyme should be eliminated from the number of diseases for which resection of the pylorus is indicated, and that operation should be replaced by digital dilatation as being more easily performed and less dangerous in results.

3. Experiment proves that continued sutures (in the stomach) are permanent and efficacious, and that the action of the gastric juice upon them has no injurious effect.

4. Experimentation shows, also, that incisions made (in the stomach) for the extraction of foreign bodies, should immediately be sutured, so that the risk of establishing a gastric fistula, which is frequently dangerous and rebellious to treatment, may be avoided.—*Revue Médical*, Jan. 10, 1885.

SUGGESTIONS AS TO THE METHODS OF USING COCAINE.—DR. E. R. SQUIBB in *Ephemeris* of January, 1885, makes the following practical suggestions as to the manner of using cocaine for local anæsthesia:

In applying the solution to other mucous membranes than those of the eye, a camel's-hair pencil has generally been used, but under the very best management, and upon surfaces made as dry as possible, the pencil leaves but a very small quantity upon the surface, and it is not to be expected that complete anæsthesia is easily attainable in this way, however frequent the applications, and a profuse use of the agent does no good, since it immediately runs off the surface. Neither is it of any use to apply it to surfaces coated with secretions, no matter how thin the coating, if they are of the glairy kind, since efficient contact with the surfaces is then impossible, and the waste is as complete as an application to the skin covered by epidermis. So, on its application to painful ulcers, burns, etc., care must always be taken to obtain contact of the solution by having the surfaces perfectly clean. In applying it to denuded surfaces and to mucous membranes which will admit it, the best method is to have some light covering of the desired shape and size, that will hold the solution For this purpose fine thin cotton or linen fabric is much better, and nothing can be better than well-worn fine handkerchief material. This is sufficiently absorbent to hold the dose, and thin enough to avoid waste, and to be closely applicable to irregular surfaces. When touched in situ, for resupply, the brush should be applied to the upper edge of the tissue, so as to run down through it before draining off, and the smallest possible quantity should be applied at a time if waste by running off would be avoided. Much solution is wasted by the use of camel's-hair pencils many sizes too large. Indeed, it is hardly possible to get a brush too small to convey this solution economically, unless large surfaces are to be coated. In the use of a brush for applying the solution it should never be dipped into the vial of solution, because it carries back secretions and excretions, which rapidly spoil the remaining solution, and it is thus wasted. The proper way is to drop out the quantity to be used on the bottom of a tumbler or wine glass, and dip the brush in this until it is all used, never putting any back into the vial. In this way any desired quantity may be used and none be spoiled or wasted. From reading the published accounts of the application of the solution, it seems highly probable that more of it is wasted than is really utilized, and if this waste could be stopped without increasing the number of failures, the price of the chemical would soon come down.

THE GONOCOCCUS AND THE ABORTIVE CURE OF BLENORRHAGIA.—DR. DOMENICO BARDUZZI, in the *Tip. Vanucchi*, 1884, admits the causative relation of the gonococcus in the production of blenorrhagia, and considers injections of corrosive sublimate of abortive value in the disease if practised on the first or second day after its appearance. The strength of the solution recommended is from 1 : 9000 to 1 : 3000, according to the toleration of the patient. He recommends injection of one-half of a teaspoonful of the fluid into the urethra three times a day, and cautions against making the least effort to throw it into the deep portion of the urethra. Of six cases treated, only one passed to the acute period. In the five remaining cases, the disease aborted completely within four days. For sake of precaution, the injection was continued a day longer.—*Giornale Italiano delle Malattie veneree e della Pelle*, Nov. and Dec. 1884.

THE MEDICAL NEWS.

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SATURDAY, FEBRUARY 28, 1885.

THE TUBERCLE BACILLUS AT THE ROYAL MEDICAL AND CHIRURGICAL SOCIETY.

PERHAPS the most important discussion which has taken place in the English language, on the subject of the bacillus tuberculosis, occurred recently at the Medico-Chirurgical Society of London, when DR. PERCY KIDD read a paper "On the Distribution of Tubercle Bacilli in the Lesions of Phthisis," in the presence of the leading members of the profession of that metropolis. Neither the paper nor its discussion can be said to clear up all the uncertainty which must be admitted to hang about the question of the relation of the bacillus to the processes which previous to its discovery were commonly acknowledged to be phthisical, if not tubercular. At the same time, both have added facts which have an important bearing on the subject.

Dr. Kidd's paper is based upon a study of 80 cases, of which 75 were tuberculous, 2 sacculated bronchiectases, 2 malignant disease of the lung, and 1 dysenteric ulceration of the intestine. In the fetid contents of the bronchiectatic cavities were found numerous micrococci but no bacilli. Bacilli were invariably found in pulmonary cavities, and in nearly all cases in softening caseous material, provided that the morbid processes were associated with a tuberculous affection. Now, this statement would seem to imply that certain caseous processes were not associated with a tuberculous affection, while we are informed that all 75 were tuberculous affections. Dr. Kidd does not give his criteria of a tuberculous affection, nor whether he would consider the form of disease known as fibroid phthisis a "tubercular affection," although he says where fibroid induration was pronounced, no bacilli were found, showing that he included among the 75 cases some with fibroid induration. He also says that the distribution of the bacilli in the so-called caseous pneumonia appears to be very irregular. Thus, generally speaking, where the caseation was uniform and firm, no bacilli were found. But where the caseous process was commencing, or where softening was going on, and microscopical cavities were present, bacilli were often seen in great numbers; and the older the caseation the less the chances that bacilli would be present.

While there is no country in which the bacillus doctrine has received more general support than in England, it is plain, from the discussion which followed the reading of the paper, that English physicians are not all in accord on the subject. Some very interesting generalizations were however made. Dr. Creighton remarked that in the large class of cases included in the term granulomata, that is, leprosy, glanders, lupus and syphilis, rod-shaped bacteria not easily distinguishable, have been found. From this he deduces the fact that granulomata form a suitable nidus for the development of rod-shaped organisms. He was not satisfied that the connection between tuberculosis and the bacilli, to the exclusion of all other organisms, had been shown. Dr. Theodore Williams thought that Dr. Kidd's researches confirmed Koch's observations in every point.

Sir Andrew Clark, on the other hand, could not accept the statement that all the phenomena were to be accounted for by the tubercle bacillus, and thought such a theory at variance with clinical facts. If by phthisis was to be understood that assemblage and progression of symptoms associated with or dependent upon ulcerative or suppurative destruction of more or less circumscribed non-malignant deposits in the lung, then he was satisfied that there was a considerable number of cases within that definition in which no bacilli were found. In fibroid lungs with ulceration, bacilli could not be found either in the expectoration or in the tissues or cavities. He was not prepared to accept the view that organisms are always found in acute tuberculosis. In a considerable number of cases of caseous pneumonia, no bacilli are found until after softening has set in. In a few cases, on the other hand, they do occur, and this was to him a great difficulty in the way of accepting the theory of a causal relation. The life history of caseous pneumonia was so distinct from that of tubercular phthisis that the occurrence of the same bacillus in both forms of the disease tends to diminish the importance of the role played by that organism.

Dr. Green held that Dr. Kidd's paper is an addition to the ever-increasing accumulation of facts which tended to show that the bacillus was present in all those pulmonary lesions which were considered phthisical, and absent in others. Under the term

phthisis he included all cases of pulmonary disease which possessed (1) lung consolidation generally more or less apical; (2) lung consolidation which tended to undergo softening and disintegration; and (3) lung consolidation which possessed infective properties. From phthisis he would exclude chronic basic pneumonia, stone-mason's phthisis, etc., inflammatory disorganization from pressure on a bronchus, and syphilitic lesions.

Dr. Wilson Fox, whose comparatively recent retraction of his previous views upon tubercle—which were based on original experiments—added great weight to the bacillar doctrine, said that the old dicta about giant cells had passed away, and it is possible that even the bacillar doctrines may not be final. There is good evidence that tubercle is the only inoculable thing which produces tubercle, but it is a very grave step beyond that to say that phthisis is contagious. Dr. Reginald Thompson concluded that it is still questionable whether bacilli alone produce and propagate the disease. Dr. Moxon, on the other hand, had always felt a belief in the unity of phthisis, though he did not know its cause. Now the factor is revealed which makes phthisis one and the same thing. Dr. Moxon, too, makes the first suggestion we have yet met as to the duration of the existence of a tubercle, which is "probably three months, while that of a phthisical patient might be twenty years."

Dr. Watson Cheyne thought that bacilli might sometimes be overlooked in fibroid cases, while Dr. Heron declared they do occur in the sputum, and careful and systematic analysis discerns them sooner or later. Dr. Macnamara has failed to find the bacillus in the earlier stages of tubercular disease of bone, whereas in cases which have gone on to suppuration, the bacillus may generally be found, especially when there is a sinus communicating with the external air. He is, therefore, inclined to believe that tuberculosis of bone can originate without the presence of tubercle bacilli, and he does not consider the presence of air essential.

There is reason to believe from the above that we have been too hasty in discarding the definition of phthisis which was so generally received up to a couple of years before Koch's discovery was announced—the view which made several varieties of phthisis, of which but one was tubercular. It was the influence of the German school which shortly before Koch's discovery caused this view to be discarded and that to be substituted which made all phthisis tubercular. To us it would seem that Koch's discovery and its results tend to destroy the unity of phthisis, at least unless the clinical aspects of the disease are to be totally ignored, or until the bacillus is found in all the conditions clinically known as phthisis.

DISTAL LIGATURE FOR INNOMINATAL ANEURISM.

AT a recent meeting of the Royal Medical and Chirurgical Society, the proceedings of which are recorded in the *British Medical Journal* for January 31, 1885, MR. BARWELL read the particulars of the case of a woman forty-eight years of age, in which he successfully tied the carotid and subclavian arteries for aneurism of the innominate. At the expiration of five months the cure was found to be permanent, no enlargement could be detected at the original site of the tumor, percussion was clear, the respiratory murmur was distinct, and there was no pulse in any branch of the ligated vessels.

In the discussion which followed the reading of the paper, Mr. Holmes stated that simultaneous deligation was unnecessary. He preferred to tie the carotid first, as ligature of the subclavian might subsequently not be needed. In cases in which the stress of the aneurism falls chiefly on the subclavian part of the sac, it might be preferable to tie that vessel first; but, as a rule, ligation of the carotid completely arrests the current of blood, since it gives off no such considerable branches as does the subclavian. In addition to these points, he urged that ligature of the subclavian adds greatly to the danger of the operation.

It will thus be seen that the views of two of the most eminent authorities on aneurism at the root of the neck are in direct conflict. Hence, it may not be unprofitable to recall the statistics of the two operations, both as regards their direct results and ultimate effects, which we have endeavored to bring up to the present date.

The carotid and subclavian arteries have been tied for aneurism of the innominate alone at least 19 times, the operators having been Barwell in two cases, and Rossi, Lane, Holmes, McCarthy, Ensor, Elliot, Weir, King, Little, Stimson, Durham, Pollock, Browne, Cameron, Von Bergmann, Rosenstern, and Banks each once. Of the 19, 11 recovered, and 8, or 42.10 per cent., died as the direct result of the procedure. Of the 11 recoveries, 3 died, at intervals varying from fifty-five to one hundred and eleven days, from rupture of the sac; 2 died of phthisis at the expiration respectively, of twenty-two and forty months; 5 were living at the end, respectively, of three months, three months and a half, five months, ten months, and twenty months; and in 1 the exact date is unknown.

The carotid alone has been tied by Mott and Nussbaum each twice, and by Evans, Neumeister, Morrison, Scott, Dolhoff, Fergusson, Porta, Hutton, Vilardebo, Wright, Ordile, Broadbent, Butcher, Newton, Pirogoff, and Bryant each once. Of the 20, 5 recovered, and 15, or 75 per cent., perished. Of the 5 recoveries, the patient of Evans lived nearly forty years, 1 was living two months and a half subsequently, 1 died some time after of rupture

of the sac, 1 succumbed from asphyxia in seven months, and 1 perished suddenly in twenty months.

These data certainly do not sustain the views of Mr. Holmes. The immediate mortality of carotid ligature is greater by 33 per cent. than is that after ligature of both vessels, and it does not appear that the former operation is more important and influential in lengthening life than is the latter. Thus, there were only two permanent recoveries after carotid deligation, the cases being those of Morrison and Evans, whose patients lived respectively for twenty months and nearly forty years; while there were five final recoveries after ligature of the carotid and subclavian, namely, the two patients of Barwell, and the single cases of Cameron, Little, and Stimson, of which three were living, respectively, at the end of five, ten and twenty months, while the other two died of phthisis at the expiration, respectively, of twenty-two and forty months.

In the present state of our knowledge it may, therefore, be declared that the views of Mr. Barwell on the treatment of innominatal aneurism are entitled to greater confidence than are those of Mr. Holmes, since carotid deligation shows 33 per cent. more of deaths, and 16 per cent. less of final cures, than does simultaneous ligation of the carotid and subclavian.

THE PREVENTION OF INFECTIOUS AND CONTAGIOUS DISEASES IN SCHOOLS.

THE Medical Officers of Schools Association, which was formed in England in 1884, has just issued a code of rules for the prevention of infectious and contagious diseases in schools, which contains some valuable suggestions, and which is important as being a formal expression of opinion on the part of medical men and sanitarians who have specially investigated the subject of school hygiene.

The first section relating to general hygiene gives the usual advice with regard to house-drainage, dust-bins, etc., and advises that the medical officer of the school shall inspect the premises at least once a month, and that he should make an annual return of the sickness in the school to the governing body.

With regard to the length of quarantine that should be required if a pupil has been exposed to an infectious disease, it is stated that this depends chiefly upon whether the school has or has not that very valuable safeguard—a disinfecting apparatus; which apparatus should be large enough to hold a full-sized mattress. If the school possess such an apparatus, then the quarantine should be a day or two more than the longest known period of incubation of the disease in question; if not, the period should be longer still, as there is no definite time when the clothing is certain to be free from infection.

The following quarantine times, after exposure to infection, are recommended as safe if thorough disinfection be carried out on the pupil's return to school:

Diphtheria . . .	12 days'	quarantine.
Scarlet fever . . .	14 "	"
Measles	16 "	"
German measles . .	16 "	"
Chickenpox . . .	18 "	"
Smallpox	24 "	"
Mumps	24 "	"
Whooping-cough . .	21 "	"

Disinfection at home, it is urged, should not be relied on, but, immediately on the pupil's return to school, he should be washed with carbolic acid soap (ten per cent.) from head to foot, in a hot bath; and that clothes, books, and *everything* brought back by him, should be completely disinfected.

With regard to the question, "when may a pupil who has had an infectious disease go home or rejoin the school," the following are recommended as safe rules, viz.:

Scarlet fever—in not less than six weeks from the date of the rash, *if* desquamation have completely ceased, and there be no appearance of sore throat.

Measles—in not less than three weeks from the date of the rash, *if* all desquamation and cough have ceased.

German measles (Rötheln, or epidemic roseola)—in two or three weeks, the exact time depending upon the nature of the attack.

Smallpox and chickenpox—when every scab has fallen off.

Mumps—in four weeks from the commencement, *if* all swelling have subsided.

Whooping-cough—after six weeks from the commencement of the whooping, provided the characteristic spasmodic cough and the whooping have ceased; or earlier, *if* all cough have completely passed away.

Diphtheria—in not less than three weeks, when convalescence is completed—there being no longer any form of sore throat, or any kind of discharge from the throat, nose, eyes, ears, or other parts, and no albuminuria.

These rules are prepared more especially for the use of boarding-schools, but in most respects they are applicable to day-schools as well, and there can be little doubt of their good effects if properly carried out.

THE NATIONAL MEDICAL LIBRARY.

THE condition and prospects of the legislation now pending in Congress for the providing a proper fire-proof building for the Army Medical Museum and Library are satisfactory.

On the 16th inst. the House passed a bill appropriating two hundred thousand dollars for such a building, to be constructed upon the Government

reservation, in the vicinity of the National Museum and Smithsonian Institution. To this the Senate will not agree, because it has taken the position that no more buildings shall be placed on Government reservations, but is willing to appropriate money to purchase a site—to which last proposition Messrs. Randall and Holman in the House will not agree. Meanwhile the Librarian of Congress and his friends, who want to sweep the Library of the Surgeon-General's Office into the proposed National Library, of which the said Librarian is to be in charge, are doing their best to prevent the reaching any common ground of agreement by the House and Senate.

In the debate in the House upon the passage of the bill it was opposed by Messrs. Potter and Nutting, of New York, on the ground that the books should be placed in the National Library. Mr. Potter objected on the ground that "this library and museum will end at last in a staff of officers to take care of it, and at the national expense, and an annual appropriation will be demanded to support and continue it for ever." And why not, Mr. Potter? Is there any other way of expending a similar amount of public money that will do one-tenth of the good to the people that the small annual appropriations for this library and museum have already done and will continue to do? In what way will there be any saving of expense by placing these books under the charge of the Librarian of Congress? Mr. Potter furthermore remarked that he "did not believe that we are to gain by preserving the relics and bones or wounds caused by the war, and that he wished they were all buried and hidden out of sight."

We presume that it is hardly necessary to explain to the readers of THE NEWS the short-sightedness of such a policy as this. Nothing will be saved by merging these collections with others. If it was only a question of four or five thousand volumes of books and a few hundred specimens, something might be said as to the economy of joining them with others—but when it comes to a hundred thousand volumes and a large anatomical and pathological museum, the case is altogether different. It will cost as much to care for them properly in a large building as in one devoted especially to them—nay, more, if the former is to be of marble and the latter of plain brick as proposed. This, however, is a consideration of very minor importance.

The merging of our great medical library, of which the medical profession of this country are now so justly proud, and in which they are so deeply interested, would be a calamity which we should endeavor to avert in every possible way. The utility and prosperity of the National Medical Library and Museum depend largely upon their being kept together, and preserving their individuality under skilled management. No greater mistake could be made by Congress than to destroy this individuality, and with it the strong helpful interest now taken in these institutions by the profession. This is one of the points upon which all medical men are agreed, and we do not doubt that they will at once exert both their individual and collective influence to prevent the carrying out of such a scheme.

If the bill to provide a plain but commodious fireproof building for our National Medical Museum and Library fails this session, it will be due to a few ambitious, selfish men who would ruin what they cannot rule, and the physicians of this country must see to it that the members of the next Congress are so instructed that there shall be no possibility of failure next time.

Meanwhile, we still hope that the present Congress will do its duty in the matter, which is one of the greatest possible importance to medical education and medical science, and, therefore, to all who have need of the services of skilled physicians.

SOCIETY PROCEEDINGS.

NEW YORK ACADEMY OF MEDICINE.

Stated Meeting, February 19, 1885.

THE PRESIDENT, A. JACOBI, M.D., IN THE CHAIR.

DR. GASPAR GRISWOLD read a paper on

ELECTRICITY AS A STIMULANT IN CARDIAC AND RESPIRATORY FAILURE.

Electricity, he said, has long been conspicuous among the agents that are resorted to for the relief of sudden prostration attended with cardiac and respiratory failure, such as is apt to occur from the use of such poisonous drugs as opium and chloroform; and the object of the paper was to inquire how far the methods employed were in accordance with what is known in regard to the physiology of the heart's action. Having referred briefly to the effects of the two currents, the galvanic and the faradic, he proceeded to ask, Can electricity be so applied as to stimulate the heart directly? In order to decide this, he said it was necessary to glance at the mechanism of the heart's action; and in doing this, he referred particularly to the retarding effect exerted by the pneumogastrics as opposed by the action of the accelerators of the heart. The action of the pneumogastrics was always inhibitory, while the accelerator nerves increase the rate of the cardiac pulsations at the expense of force. It was evident, therefore, that in order to stimulate the heart it would be necessary to apply the electricity to the organ itself. As a matter of fact, however, it was found that when one or both electrodes were placed over the præcordial region, the heart-beat was retarded. In order to illustrate the effect of doing this, he would refer to two cases. The first, reported in the *Philadelphia Medical Times* of June 4, 1881, was one of death from cardiac failure after etherization. The pulse was feeble and beating 180 to 200 per minute, while the respirations were quiet and regular, numbering about 22. Hypodermic injec-

tions of tincture of digitalis and whiskey, and of digitalis and sulphate of atropia were given, in addition to an enema of whiskey. "Then," the account goes on to say, "the faradic battery, which had been brought to the bedside, was suggested as a remedial agent, but as the respiration was continuing comparatively unaffected, it was only applied for a short time, with one pole to the neck, and the other in the præcordial region." The patient continued to sink, notwithstanding the further injection of the remedies mentioned, and died a little more than an hour after the anæsthetic was withdrawn. The idea here, said Dr. Griswold, was to stimulate the heart; but the reverse effect was noticed, and the use of the battery might readily have caused the patient's death.

The second case, reported in the *British Medical Journal* of July 2, 1881, was one of poisoning by bitter almonds. When seen, the patient was quite insensible and collapsed; breathing gasping and labored; he was cyanotic; the pulse was hardly perceptible, rapid and flickering, and the heart's apex-beat weak and rapid. The stomach-pump was at once used, and the stomach was then washed out with warm water, after which the patient became more collapsed, and his radial pulse almost ceased. The battery was now used for ten minutes—one pole over the apex of the heart, the other on his neck, over the course of the pneumogastric nerve. In twenty minutes, liquor ammoniæ fortior was given as an inhalation, and the current from twenty-five cells of Leclanché's battery applied as before, contact being made and broken with the inspirations. The battery and ammonia inhalations were then continued alternately, at first every ten minutes, and afterwards every half hour, for about four hours. The patient recovered, and this was attributed mainly to the employment of the electricity. It was a fact, however, that the current from a battery of twenty-five cells applied to the pneumogastric would kill a large dog, and that that from six cells would cause faintness in human beings; and therefore this treatment was improper. The recovery might be explained on one of two suppositions: either the battery was out of order, as batteries are very apt to be when called into sudden requisition, or else the pneumogastric had already become paralyzed, paralysis of this nerve being one of the results of hydrocyanic acid poisoning. It was stated in the report that the bitter almonds were eaten three hours before the electricity was applied, which would have given time for this effect to be produced. The application of electricity in this way was, then, the reverse of stimulating in its effect upon the heart, and involved great danger to life if a strong current was employed.

In regard to the respiratory function, he said that the stimulation of the phrenic nerve caused contraction of the diaphragm, and it was perfectly rational, therefore, to apply electricity to the neck for the purpose of counteracting respiratory failure. This might be done in two ways, either by applying a mild current continuously, or by employing one of sufficient strength to excite at once a deep inspiration, and repeating it at intervals of three or four minutes. Good results had been reported from its use in this way in opium poisoning, but the effect was always due to the stimulation of the phrenic nerve alone. Theoretically it was impossible to apply a current to the phrenic without affecting the pneumogastric also, because they lay so close to each other in the neck. The function of the pneumogastric was to retard or even stop the heart's action; but as in opium poisoning this nerve was apt to be paralyzed, electricity could be applied to the neck in this condition with a prospect of its affecting only the phrenic nerve. He knew of one case of opium poisoning, however, in which instant death was produced by the sudden application of a current in this locality for the purpose of stimulating the phrenic, through the effect that it had upon the pneumogastric. The conclusions which he derived from the foregoing considerations were: 1. That electrical stimulation of the phrenic nerve is liable to stimulate the pneumogastric also; 2. That only mild currents should be employed; and, 3. That especially should the sudden application of a strong current in the neck be avoided.

The circumstances under which his attention was first directed to this subject were the following: A patient that he was attending got an overdose of tincture of aconite root through the druggist's mistake in neglecting to add the water in a prescription which he had ordered containing this drug. The application of electricity to the neck, on account of the embarrassment of respiration present, was advised by Prof. A. A. Smith, who was called in consultation; but Dr. Griswold opposed this on account of the bad effect it might have on the heart's action through the stimulation of the pneumogastric nerve in addition to the phrenic. On account of the interest in the matter which this case excited, he determined to make some investigations himself, and, therefore, performed a series of experiments on dogs. He first applied a faradic current to the pneumogastric, and found that it appreciably slowed the action of the heart, as indicated by a Marey's cardiometer inserted into the carotid. The animal's system was then brought under the influence of aconite; when it was found that no effect whatever was produced upon the heart by the same strength of current. Even when the current was increased to the full strength, there was still no effect; the explanation being that the pneumogastric was paralyzed by the drug. He did not wish to claim any originality for this experiment, which he repeated with two other dogs, as it had been performed by others previously; but he was glad to know that the results which he obtained were corroborated by the observations of other investigators. The deduction that he drew from it was that there is no danger from electricity in aconite poisoning, and it is, therefore, perfectly safe to apply it to the phrenic nerve. Consequently he believed that Prof. Smith had been right in advising its use in the case to which he had referred.

The next experiments were with dogs under the influence of chloroform. A faradic current was applied to the pneumogastric, and the effect was instantaneous. In one case the heart stopped in diastole, and did not beat again, and while death occurred less promptly in the others, the result was the same. Deduction: The pneumogastric retains its excitability in chloroform poisoning, and it is, therefore, extremely dangerous to apply electricity to the neck in this condition. In asphyxiation by ether, he found that the heart could stand stimulation of the pneumogastric as well as in health, and he concluded, therefore, that it was safe to stimulate the phrenic nerve to a certain extent in this

condition. Chloroform was a direct cardiac depressant, while ether was not. In opium poisoning, when well marked, the pneumogastric was paralyzed, and there was, therefore, less danger in stimulating that nerve in this condition than in health. When morphia was injected into a vein, however, he found that the case was different, and the deduction that he made from this experiment was that when morphia is injected into a vein, the heart is easily depressed by the application of electricity to the pneumogastric.

His general conclusions were as follows:

First. Electricity cannot be applied clinically in such a way as to stimulate the heart, literally speaking.

Second. The application of one pole of the battery to the neck and the other over the præcordial region, when the pneumogastric is not paralyzed, may result in death.

Third. The stimulation of the phrenic nerve also stimulates the pneumogastric, on account of the proximity of the two nerves.

Fourth. The liability to depress the heart from stimulation of the pneumogastric in connection with the phrenic is not great in aconite, ether, or opium poisoning, on account of the paralysis of the pneumogastric produced by these drugs.

Fifth. In heart-failure from chloroform, and the injection of morphia into a vein, the application of electricity to the neck is strongly contraindicated.

Sixth. Under no circumstances should a current strong enough to cause muscular contraction be applied suddenly to the neck.

DR. A. D. ROCKWELL remarked that what he should have to say would be, in the main, confirmatory of the views expressed by the author of the paper. Electricity had long been recognized as of special value in stimulating the respiratory function, rather than the action of the heart. A few years ago it had been claimed by certain authorities that the action of the two pneumogastric nerves, on the two sides of the body, was somewhat different in character; the one affecting the respiration more, and the other the cardiac functions. Clinical observation in the use of electricity, it seemed to him, had afforded a considerable amount of corroborative evidence of the correctness of this hypothesis. One of the most important things about the action of electricity, and one which was apparently very imperfectly understood by medical men, as a rule, was its secondary influence. As one who had paid much attention to the subject of electro-therapeutics, he fully appreciated the force of the statements made by Dr. Griswold. Theoretically, his views were undoubtedly sound; but, as a matter of fact, he did not believe it was necessary to use electricity with such extreme caution as had been advocated in the paper. Some time ago, Brown-Séquard, Anstie, and others, had laid stress upon the great danger of applying electrical currents to the central nervous system through the pneumogastrics. He could easily understand why such unpleasant results as they mentioned might be apprehended, reasoning from the physiological functions of the pneumogastric; but, practically, he did not believe the application was so dangerous as was inferred. In this connection he related a case he had seen two years ago, in which he was called to attend a patient who was in an unconscious condition from some narcotic poison, afterwards ascertained to be opium. When first seen, the respirations were about ten to the minute, and the pulse about forty. The application of the faradic current to the phrenic nerve in the neck had a very striking effect, and immediately the respirations went up to normal. The pulse also rose under the action of the electricity, and for two hours the application was kept up intermittingly. At the end of two hours, however, both the pulse and the respiration began gradually to decline, notwithstanding the continued use of the agent, and after four hours of treatment the patient seemed to be in exactly the same condition as when first seen. Under these circumstances the case was believed to be utterly hopeless, and further treatment was abandoned; but, strangely enough, reaction set in after a time spontaneously, and the patient recovered. The practical lesson to be derived from this case was, he thought, that as long as there was any breath at all left in a patient's body, the use of electricity should be kept up. Here undoubtedly the employment of the current for four hours had tided over the system under the depressing influence of the opium until nature had a chance to assert her powers.

DR. FORDYCE BARKER remarked that there were two or three collateral points of practical interest in connection with the paper upon which he should like to say a few words. One was the comparative influence of ether and chloroform on the heart, and he thought there were certain clinical facts bearing on this which would seem to militate against what might be inferred from the paper. In obstetrics he had found that chloroform had a very different effect from that which it had when given as an anæsthetic in surgical practice, and in a paper which he had written many years ago, he had expressed the opinion that ether was much the safer agent to use in general surgery, and chloroform the safer in obstetrical cases. The reason that he assigned for this was that in surgical practice the anæsthetic was given to anticipate suffering, while in the case of the parturient woman it was used for the relief of pain already existing. Under the latter circumstances, he believed the system would tolerate the depressing influence of a drug which it might not in ordinary conditions.

It had long been an accepted point in the opinion of the profession that it was unsafe to administer an anæsthetic when there was organic disease of the heart; but it was, at least, twenty-five years since he had ceased to believe that the existence of such disease was a contraindication against the use of chloroform in pregnant women. On the contrary, the presence of cardiac trouble was to him an indication which induced him to resort to this anæsthetic at an earlier period than usual. Ether, however, was not safe, and its use in such cases was to be condemned. That chloroform was a protection rather than a dangerous or injurious agent, under the circumstances, was strikingly shown in the case of a patient in the lying-in wards of Bellevue Hospital, which he had seen many years ago. When this woman came into the wards, expecting to be confined, she was found to be in the advanced stages of valvular disease of the heart, with marked dropsy, dyspnœa, etc., and her case was watched with much interest. One night he was summoned hastily to the hospital by the *interne*, as labor had commenced, and she was in such an alarming condition that fears were entertained that she would not survive delivery. She was suffering

from great depression of the vital forces, and at every pain the pulse would fail entirely. Under these circumstances, he concluded to try the effect of chloroform, and immediately the patient's suffering was completely relieved. The administration of this was kept up for some time, and while the patient remained perfectly comfortable, the uterine contractions went on in a very satisfactory way. The pulse and the whole condition steadily improved under its use, and the delivery was accomplished without difficulty; the uterus also contracting well afterwards. She rapidly improved after her labor, and when she left the hospital her friends remarked that she was looking better than she had for a long time previously. During pregnancy he believed that there was a normal physiological hypertrophy of the heart, and at the time of parturition he thought there was a saving of vital or nerve power secured by the administration of chloroform, which more than counterbalanced any tendency on the part of the drug to produce failure of the heart. That the same thing would be true in case of ether being substituted for chloroform, he did not believe; and he would repeat, therefore, that obstetricians should never use any other anæsthetic than chloroform. Within the past two years, a physician in Edinburgh, whose name he could not just now recall, had written a book in which very much the same views as his own in regard to the position of chloroform in midwifery practice were expressed.

DR. R. W. AMIDON said that he agreed fully with the opinions expressed in the paper, but regretted that its author had not pointed out what remedies could be depended upon to take the place of electricity, which he had so admirably shown did not deserve the position in the class of cases referred to which it had hitherto held in the opinion of the profession. He could see why Dr. Rockwell, who is a specialist, and therefore exceedingly skilful in the application of electricity, should take a somewhat different view of the safety of the agent from that entertained by the reader of the paper, who is a general practitioner; but in the pressing circumstances of the emergency, in which physicians were ordinarily called upon to treat a case of opium, ether, or chloroform narcosis, for instance, the chances were that the electricity would by no means be applied in an efficient or judicious manner. It is not a simple, but a powerful and dangerous remedy, which comparatively few medical men know how to use intelligently. On the other hand, artificial respiration is a means of treatment which all can employ without danger of doing any harm, and which is very likely to be followed by good results in all cases of respiratory failure; while it is a fact which ought not to be forgotten, that in many cases the cardiac failure is secondary to this.

DR. L. PUTZEL was of the opinion that, while the physiological aspects of the subject were well stated in the paper, the deductions from experiments on animals were not trustworthy. In the human subject no method had yet been devised for applying electricity directly to the pneumogastric nerves; so that the best electrotherapeutists spoke now simply of galvanization of the neck. With regard to the evil effects of the application of a strong current to the neck, he thought these had been exaggerated in the paper, since he himself had frequently used a current from a twenty-five cell battery, in perfect order, in this situation, and was still constantly doing so in cases of exophthalmic goitre, without producing any appreciable effect upon the heart or respiration. From a clinical standpoint, the subject is one which he thought needed revision. As had been well said, physicians are apt to be extremely careless in this matter, and in the report of a case it would not do simply to state that electricity was applied.

DR. J. LEONARD CORNING said he must take exception to the statement made by the last speaker, that it was impossible to apply faradization to the pneumogastric nerves, and proceeded to describe a method of his own by which he claimed to have successfully done this. This, he believed, was proven by the effects produced, and he said that in a case of exophthalmic goitre he had reduced a pulse of 140 to 80 within a period of three months.

DR. ANDREW H. SMITH remarked that in opium poisoning the great difficulty was usually in the respiratory function, and therefore the principal object was to stimulate the failing respiration. The fact could not be denied that it was usually possible to produce a full respiration by the application of electricity to the phrenic nerve; and if this was repeated about sixteen times a minute, it was apt to be followed by good results. It should not be done oftener than this, though there was a strong temptation to push the application; and in this way the cardiac failure might be secondarily arrested. Electricity was also useful as a general stimulant in opium poisoning. Flagellation was often injudiciously practised, and from the abuse of this there was liable to arise the added danger of shock.

DR. PUTZEL explained that he did not wish to be understood to say that galvanization of the neck *never* slowed the pulse, but that in his own experience he had never observed this effect.

THE PRESIDENT said that in former years he had used electricity a great deal in cases of cardiac and respiratory failure, such as was seen in opium poisoning, but more especially in the asphyxia of the newly born. He had found that if the current is used more or less continuously for any length of time, while at first these functions are stimulated, the increased activity is afterwards followed by paralysis; and that this effect is reached the more speedily the closer to the phrenic and pneumogastric nerves he applies the electricity. This experience taught him, therefore, that the best practical way of using the agent is not to confine the current to the nerves, but to apply it generally to the muscles. His method is to keep one electrode on the body and then bring the other in contact with the skin in a succession of quick applications to the various parts of the surface, and this is the plan that he would advise in cases of opium poisoning. The shock thus received is often sufficient to excite the respiratory function, and he quite agreed with Dr. Amidon that the failure of the heart is often secondary to that of the respiration. The electrode used for those general applications upon the surface should preferably be a faradic brush.

DR. GRISWOLD said that in his paper he had dwelt upon three points. The first was that the heart cannot be reached in such a way as to be stimulated efficiently and certainly. He would not have thought it necessary

to lay any stress upon this had not the two cases to which he had referred at the outset, published in medical journals of high reputation, led him to believe that such an opinion might be entertained by a certain number of physicians. The second point was to call attention to the fact that the pneumogastric and phrenic nerves lay so closely together in the neck that the stimulation of one was impossible without that of the other in addition. In opium poisoning, where the patient was well narcotized and the heart's action had become frequent, we could stimulate the pneumogastrics with impunity, on account of their state of paralysis. In the case of instant death to which he had referred (of which he had learned from Dr. Janeway, who had expected to be present and speak more particularly of it), the pneumogastric had not yet become completely paralyzed at the time the current was suddenly applied to it. It was, therefore, confirmatory of the results shown by his own experiments on dogs; so that he believed that the reason why more patients suffering from opium poisoning were not killed by the application of strong currents to the neck was on account of the complete paralysis of the pneumogastric. The only point that his experiments bore on, he continued, was the exaggerated sensibility of the heart to the action of electricity. In chloroform poisoning particularly, he believed it was best to rely on artificial respiration, which was safe and efficient, while stimulation of the phrenic nerve was not safe or efficient. The hypodermic injection of ammonia, alcoholic stimulus, etc., might also be employed. In regard to chloroform, Simpson was the first to show that it was much more of a local anæsthetic than other drugs of its class. The local effect of ether was simply one of freezing, on account of its rapid evaporation.

DR. M. JOSIAH ROBERTS gave a description and practical

DEMONSTRATION OF THE WORKING OF ROBERTS'S IMPROVED ELECTRO-OSTEOTOME; A NEW FORM OF PORTABLE STORAGE BATTERY AND AN ELECTRICAL ILLUMINATING APPARATUS.

The osteotome is an improved form of the one described in the *New York Medical Record* for October 20, 1883, and consists of an electro-motor with a central shaft, to the extremity of which saws of various sizes can be attached. It is claimed that perfect accuracy of cut can be secured with the instrument. Five seconds are required to saw through an adult femur, and Dr. Roberts stated that in his largest osteotomy at one sitting he had made six sections of bone in thirty seconds. A drill can be substituted for the saw, and by this means exploratory punctures can be made into the heads of bones, answering to exploratory punctures of soft parts with the hypodermic needle. The apparatus is also provided with an electric light, which can be used when necessary; two cells of the battery being reserved for the purpose of maintaining it. Metallic retractors are employed for the purpose of protecting the soft parts while the osteotome is in operation. The improvement is the increased oxidation on the surface of the leaden plates used, and Dr. Roberts claims that it can be implicitly relied upon for all practical demands.

NEW YORK COUNTY MEDICAL ASSOCIATION.

Stated Meeting, February 16, 1885.

THE PRESIDENT, CHARLES A. LEALE, M.D., IN THE CHAIR.

DR. THEODORE R. VARICK, Surgeon-General of the State of New Jersey, read a paper on

THE PROTECTIVE TREATMENT OF OPEN WOUNDS.

However different his views may be from those entertained by a large number of surgeons, he said, they are not the inspiration of a moment, but the conviction of long-protracted observation. In grave surgical injuries the sources of trouble were, first, from shock and hemorrhage; and, secondly, from sepsis; and from time immemorial it had been the aim of surgeons to minimize the dangers of the former, and prevent the occurrence of the latter. In order to accomplish this latter end, it was desirable to protect the parts as perfectly as possible from external influences, and any method by which they could be hermetically sealed, and yet which would not result in causing irritation of the tissues beneath, would certainly be a great desideratum. In order for the production of sepsis, a solution of continuity was necessary. At first the danger was warded off by the constant exudation from the vessels of the injured parts. When this ceased, the open stomata of the vessels afforded a ready avenue for the absorption of septic material, unless something was done in the meanwhile to close or cover them.

Having read the description given by Sir James Paget in Holmes's *Surgery* of the appearances presented by open wounds, he made the following quotations from Flint's *Physiology* in regard to blood-serum: "The serum contains all the elements of the blood, except the red corpuscles and fibrin, which, together, form the clot." "It contains all the principles found in the plasma, or liquor sanguinis, with the exception of the fibrin. It can hardly be called a physiological fluid, as it is formed only after coagulation of the blood, and never exists isolated in the body." Blood-serum is largely albuminous, and it was this element, Dr. Varick continued, which he had to consider. On the coagulation of the albumen depended the success of the method of treatment of open wounds which he advocated; and since he had adopted this plan, he had yet to meet with the first case of septicæmia in his practice.

It was, indeed, to their property of coagulating this element that the bichloride of mercury and carbolic acid owed their reputation as antiseptics in surgery; but there were positive objections to both these agents, which, in his opinion, rendered them, as a rule, entirely unsafe to use. In regard to the bichloride, there was a possibility of constitutional effects resulting from its use, and several cases had already been reported in which severe tenesmus had been caused by it. Another objection to it was its irritant effect locally, and in this connection he quoted from an address made by Sir Joseph Lister at the opening meeting of the Medical Society of London October 20, 1884, in regard to the case of a lady from whom he removed the mamma and cleared out the axillary glands, as follows: "I put immediately over the wound a piece of prepared oiled silk, which I dipped in a 1 to 500 solution of corrosive sublimate; then, over this, sublimate gauze, and outside

all, abundant eucalyptus dressing. On the following day, when we changed the dressing, I found that under the oiled silk, exactly corresponding to this extent, the skin was highly irritated, and was covered with small vesicles. I also found that the inner side of the arm, where there was no wound, was in the same state of intense irritation. . . . . While this case showed that in the sublimate we had an agent that might give very beautiful results, it also indicated that we were dealing with an edged tool, which, while it might do admirable work, was very apt to cut our fingers."

In speaking of carbolic acid, he quoted Bartholow (*Materia Medica and Therapeutics*) as follows: "Applied to the external integument, carbolic acid is absorbed and systemic effects follow, the urine becoming smoky. Fatal poisoning has resulted from such applications (Köhler)." In a case of Dr. H. B. Sands, of New York, in which the patient had been subjected to the operation of amputation of the thigh for ununited fracture of the femur, for five days after the operation the urine was green in color; an effect due to the absorption by the wound of carbolic acid, and in accordance with the antiseptic method of operating (*New York Medical Record*, vol. xx. p. 151). Mr. A. Pearce Gould had reported at the Clinical Society of London, in 1879 (*Lancet*, April 26 and May 17, 1879), a case of amputation at the hip-joint, in which the patient died at the end of sixty-eight hours. At the autopsy all the veins of the left foot, leg, and thigh, were found to be filled with black coagulum, which reached into the common iliac vein; the thrombosis being due to carbolic acid poisoning. All went well up to forty hours after the operation, when suddenly and coincidently with the excretion of carbolic acid urine, the fatal symptoms set in—symptoms which were not merely local and limited to the affected limb, but general. Dr. Varick then referred to a large number of other cases in which fatal or dangerous results had followed the local use of carbolic acid, reported by both American and foreign writers. Among the former was Dr. Alfred C. Post, who reported at the New York Pathological Society, March 26, 1879, a case in which death was only averted with the greatest difficulty. A subacute abscess in the gluteal region, connected with old disease of the hip-joint, in a child five years of age, was evacuated, and the cavity injected with a solution of carbolic acid, according to the method advised by Callender; when in a few hours the patient presented the constitutional symptoms of profound carbolic acid poisoning. In an almost precisely similar case reported at a meeting of the Boston Society for Medical Improvement, held March 28, 1881, by Dr. S. H. Bradford, the patient died. The abscess contained about eight ounces of sero-purulent fluid. Carbolic spray was played upon the limb during the incision; the abscess was hyperdistended with a solution of carbolic acid of a strength of one to forty. The patient recovered quickly from the ether, but in the following night was seized with obstinate vomiting, which persisted during the morning, but stopped at noon. It returned again in the night, and the following morning the child was in a feeble condition. The urine that was passed was tar-colored, and death took place two days later, with symptoms of collapse. Having referred to some experiments on turtles and frogs, by Dr. H. G. Byer, U. S. N., which showed in a striking manner the depressing effects of carbolic acid, he said, in summing up, that both carbolic acid and bichloride of mercury were undoubtedly liable to excite toxic effects, and were therefore unsafe.

What method are we to adopt, then, in the treatment of wounds, he asked. For six years, he said, it had been his practice to use *hot water*, slightly below the boiling point, freely and continuously, until all oozing had ceased. The effect of this was to cause the formation of a protective shield from fibrogenous material which was always available. This procedure, he claimed, not only facilitated healing, but also tended to prevent shock. By the transmission of heat it assisted in strengthening the heart's action, and he had had no death from either primary or secondary shock in his public or private practice since he had adopted it, although his hospital experience had been very large. The method presented the following merits: 1. Simplicity; 2. Availability; 3. Safety; 4. Invulnerability.

In order to test the protective agency of albumen, he had performed a number of practical experiments. Among them were the following: On the 13th of October, 1884, he had taken twelve test-tubes, and, having thoroughly cleansed them, partially filled them with freshly prepared beef-tea. He then divided them into two sets. Over the mouths of five of the first six he placed linen covers coated with serum, leaving the sixth uncovered, while five of the other six were also covered, but with material that was not coated with serum. Within twenty-four hours the beef-tea in three of the second set of tubes was milky and turbid, and within forty-eight hours, the contents of the remaining three were manifestly undergoing decomposition. At the present date, four months after the serum-coated coverings had been applied, the beef-tea in all the test-tubes treated in this way was found to be as clear and fresh-looking as on the day on which it was put into them. (The test-tubes were handed round for the inspection of the members.)

He had commenced a second series of experiments on January 21, 1885, preparing seven test-tubes in the same manner as before, and covering six of them with linen coated with serum, but leaving the remaining one open. Within twenty-four hours the beef-tea in the latter was loaded with bacteria, and decomposition was advancing rapidly; but the contents of all the other tubes still remained in a perfect state of preservation. This paper, Dr. Varick remarked, in conclusion, was intended to be supplementary to one which he read in May, 1884, before the Surgical Section of the American Medical Association, on "Railroad Injuries of the Extremities, with Observations on the Site of Amputation and Subsequent Treatment of the Stump."

DR. WILLIAM DETMOLD said that he had been the more interested in the paper from the fact that the practice therein advocated had been the one which he had himself pursued throughout his whole professional career, with the exception that he had not employed the water hot, like Dr. Varick. He had never yet been converted to the germ theory, and had always condemned the use of carbolic acid and other so-called antiseptics. He had been accustomed to using simply a compress with even the largest wound. The hot water might perhaps be an advantage, but he thought that Dr. Varick had not demonstrated the fact that the water he employed

was hot enough to coagulate albumen. He did not feel certain that the albumen would not coagulate, or rather become dry, without heat in the case of wounds. In private practice, he had never seen any sepsis in connection with the latter, except in certain localities, generally the scrotum or some other part of the genito-urinary tract. What was it, he asked, that made internal urethrotomy so dangerous? A great many cases of this proved fatal, while such a result was very rare in external urethrotomy, in which we would naturally expect to find it most frequent, according to the hypothesis of atmospheric germs, on account of the free access of air to the parts. In internal urethrotomy, this was not the case; but the cutting was done in a situation where the cellular tissue was loose and the large vessels did not contract readily. He thought pus got into the veins without the agency of any germs.

In regard to water dressing for wounds, he believed that the use of even cold water might be carried to a degree that was injurious, and he had seen it almost macerate the skin. It was his custom to apply compresses now that were wrung out, so as not to drip, and in this way the possibility of macerating the epidermis was obviated. Notwithstanding the prevalence of the new fashion of antiseptics and drainage-tubes, he had always adhered to the old methods. He had been brought up to consider that in surgical practice foreign bodies should always be removed, and he did not like the idea of introducing additional foreign substances into the tissues. In regard to Dr. Varick's experiments, he thought it had not been proven by them that blood-serum, without being exposed to heat, would not have the same effect as the serum after having been thus exposed to heat; and it was his opinion that it had.

DR. VARICK said that he did not know that he was fully prepared to answer Dr. Detmold's objection. Physiologists tell us that albumen outside of the body remains fluid until it is subjected to heat or some chemical agent, having the power of coagulating it. When it is exposed without the influence of these agencies, the process which it undergoes is simply one of desiccation rather than coagulation. If there is anything in the germ theory (according to which sepsis is caused by the admission of foreign disease-bearing organisms), during the time required for the albumen to desiccate there was ample opportunity for the introduction of such germs; but if measures are taken to secure the prompt coagulation of the albumen, before the outward flow from the vessels of the injured part had ceased, such an occurrence is rendered impossible.

DR. J. W. S. GOULEY remarked that there were several points of great interest touched upon in the paper, but he would refer to only one or two of them. In regard to the treatment of wounds, it had long been agreed upon by all surgeons that, whether from fear of contamination of the injured parts or to facilitate more prompt union, it was desirable to bring about coagulation of the serum effused. Years ago, it had been his custom to apply to the surfaces of all wounds a solution of chloride of zinc, prepared in accordance with the formula of Dr. Pancoast, of Philadelphia; the object being to accomplish exactly the thing which Dr. Varick now succeeded in doing by much simpler means. With the idea of obtaining as thorough coagulation as possible, he had always waited for an hour or more before closing the wound; by which time he found that it was usually completely glazed. He had met with a certain proportion of cases of septicæmia; and in his experience profuse hemorrhage was more apt to be followed by pyæmia than any other accident or condition. Like Dr. Detmold, he was not yet a convert to the germ theory, and it seemed to him that excessive loss of blood was a much more probable factor in the causation of septic trouble than the admission of any so-called germs. He differed from Dr. Detmold, however, in believing that the pus did not enter the veins from without, but was really formed in them. When a thrombus had occurred, it was liable to undergo disintegration, and thus it was that suppuration took place in the liver and other parts. He had seen patients die, not from hemorrhage, but from the putrescent condition of the injured part, within forty-eight hours, or, in other words, before any germs could have had time to undergo their process of development, and bring about a fatal result.

But as he had said before, he had long been a believer in the prompt coagulation of the surfaces of wounds, and he could not too strongly express his obligations to Dr. Varick for the bold stand which he had taken in condemning the use of such agents as carbolic acid and the bichloride of mercury. He believed that more lives had been lost through the agency of carbolic acid than could possibly have been saved by it. The surgeon himself was sometimes the victim of the poison, and Dr. Donald Maclean, Professor of Surgery at Ann Arbor, had once suffered from bloody urine, and was really quite ill for several days after performing an operation in which the carbolic spray was used. All good surgeons, indeed, were antiseptists, but were not all Listerists. The first case of pyæmia resulting from profuse hemorrhage, which he had seen, was that of a robust young man, who had received a severe gunshot wound of the shoulder. He lost a very large quantity of blood, and, after a few days, chills set in, and he died from pyæmia resulting from thrombosis. In a large number of autopsies Dr. Janeway had found thrombi in the veins of the pelvic and other regions.

DR. C. S. WOOD, who was introduced by the Chair as having been one of the surgeons at Gettysburg, said that during the last few years the subject of antiseptics had undergone many changes, and that he believed that many of our best authorities at the present day were inclined to lay aside this bactero-mania which had been so prevalent. At the time of his experience in the army Listerism was not known, and while gangrene and sloughing were seen arising from special causes, he did not believe that these complications were due to the lack of antiseptic precautions. Among these special causes were all kinds of depressing influences. After the battle of Gettysburg more than eleven thousand wounded soldiers, both Union and Confederate, were under the care of our surgeons. The circumstances surrounding both classes were precisely alike, the surgeons making no difference whatever in the degree of care with which they attended to their injuries, yet on account of the feeling of elation after victory on the one side and of depression resulting from defeat on the other, a much larger proportion of Union soldiers recovered than of the Confederates. Again, at Fair Oaks, the next day after wounds had been received or

amputations performed, the seat of injury or stump was found to be covered with maggots of enormous size, and the terror which the men felt at the idea of having living animals thus crawling in and out of them had such a bad effect that the proportion of fatal cases was unusually large. When it had been discovered that the use of turpentine afforded an efficient means of getting rid of the maggots, the results were much more favorable; and it was also found that in the cases in which turpentine was employed, gangrene and sloughing were exceedingly rare. The effect of the weather on patients suffering from wounds was also well shown during the war. After all great battles a storm was sure to follow, to which the wounded were usually exposed with very little protection, and it was found that if the storm continued but for one day, it was decidedly beneficial in its effect, cleanliness being, as he believed, the essence of antisepsis; but if it lasted for two or three days, the results were injurious, on account of the men having to remain wet for so long.

DR. DETMOLD said that he could confirm what Dr. Wood had mentioned in regard to maggots from his experience in European hospitals. Patients were usually greatly disgusted with them; though it had been demonstrated that in reality they were utterly harmless, being simply the eggs of the fly unusually developed by heat and moisture. The moral effect of their presence, however, was exceedingly bad.

THE PRESIDENT thought that the microscope would explain one of the questions raised, and also explain the wonderful success of Dr. Varick. It was found that the blood-corpuscles became swollen from endosmosis, and that their decomposition afterwards resulted. Water at ordinary temperatures did not possess the power of coagulating fibrin, but hot water had that power. By the agency of the latter a film was formed that coated the wound and prevented both the decomposition of blood-corpuscles and the entrance of any germs from the atmosphere, granting that these were sometimes the cause of sepsis. Dr. Varick's experience had been unusually great, as he had long been connected with two of the largest hospitals in the United States, in which an immense number of acute surgical cases was treated all the time.

DR. VARICK, in closing the discussion, remarked that he had little to say in addition to his previous statements. In the paper that he had read, the results which he had met with had been alluded to only in a general way. The statistics of his amputations were fully set forth in the other paper mentioned, which he read at the last meeting of the American Medical Association. They were all major operations, and mostly in cases of railroad injuries. He had also had some major amputations since that paper was prepared, and he would now refer for a moment to these, in connection with the others. From the middle of August, 1879, up to the date of the paper, he had had at St. Francis Hospital, Jersey City, 21 such operations, and since then 1 primary amputation of the arm; making 22 in all, all of which were successful. At the Jersey City Hospital, during the period named, he had had 7 operations, and since then 4; making 11 in all, of which 1 case proved fatal. This amputation was of the thigh, and the patient, who was a broken-down tramp, died in the night from secondary hemorrhage. In private practice, he had had up to the date of the paper 3 cases, and since then 1. Of these 4, 1 died; but the stump was doing perfectly well, and the real cause of death was valvular disease of the heart. There was thus a total of 37 cases, with 2 deaths; which, he thought, was a pretty good showing for the method of treatment adopted. His success depended also on two other additional points, viz.: making the amputation through sound tissue, and keeping the stump clean.

That the hot water treatment did exert a protective power against hospitalism, he had met with several excellent illustrations in his experience. On one occasion, at the Jersey City Hospital, when the house was greatly crowded, and when there were several cases of puerperal fever in the obstetric wards and nine or ten cases of erysipelas in the male wards, two cases of railroad injury were brought in which involved amputation of the thigh. Yet both these patients made good recoveries, notwithstanding the exceedingly unfavorable influences to which they were subjected.

Referring to Listerism, he said that it had been claimed that it was a system of cleanliness; but he denied this. While he had seen enthusiastic disciples of Lister clean the limb of a child four years of age, so that no precaution might be omitted, he had also seen the most approved antiseptic dressing removed from a wound covered with the most fetid pus that he had ever seen in his life.

NEW YORK SURGICAL SOCIETY.

Stated Meeting, February 10, 1885.

THE PRESIDENT, ALFRED C. POST, M.D., LL.D., IN THE CHAIR.

REFRACTURE OF THE FEMUR.

DR. POST presented a patient, a man 22 years of age, who came under his care between two and three months ago. At that time it was five months since he received a fracture of the right thigh at the junction of the second with the upper fourth, having been struck by the paddle-wheel of a steamer, and for which he was under treatment in a Staten Island hospital for several weeks. Dr. Post found great bowing of the thighs, with the convexity outward, and with a shortening of two and a quarter inches. He sent the patient to the Presbyterian Hospital, where he refractured the bone by means of Rizzoli's osteoclast, loaned to him by Dr. Sands. It was only with a great deal of force that he succeeded in refracturing the femur. He then treated the case with extension and weight and pulley, the weight being twenty-five pounds. It was continued with side splints during a period of six or seven weeks. The patient remained in bed two or three weeks longer, when he was allowed to get up, and there is now shortening not exceeding half an inch. There is a little fulness in the region of the callus, but there is no apparent deformity, except slight anterior convexity. In refracturing, the bone gave way very gradually, the arch of the limb yielding slowly for some time before mobility was perceived. The patient now stands and walks firmly, and there is nothing in his appearance or movements to indicate that shortening exists.

In response to a question asked by Dr. Markoe, Dr. Post said that the inflammatory reaction following the

operation was no more than after the occurrence of ordinary fracture.

ADENO-CARCINOMA OF THE RECTUM.

DR. GERSTER presented a patient, a man 27 years of age, who, since he was eight years old, had suffered from prolapse of the anal mucous membrane at each stool. For the relief of this, he had used injections of cold water for a very long time, the introduction of the nozzle of the syringe being done by his right hand. During the last year and a half, he had noticed frequently more or less profuse hemorrhage with each passage of the bowel. In November last, the patient came under Dr. Gerster's care at Mount Sinai Hospital, where he diagnosticated a tumor on the left side in the posterior portion of the rectum, overlapping somewhat the mucous membrane, but still sessile and ulcerated in the centre. It bled on the slightest touch, and occupied about two-fifths of the circumference of the gut, extending upward so far that it was just possible to touch healthy mucous membrane with the tip of the ind finger. It was freely movable. It was evidently a malignant growth, and an operation was performed for its removal by first making an incision from the anal margin to the coccyx and excising the tip of the coccyx. The sphincter being perfectly healthy, the mucous membrane covering it was dissected up, and then the lower margin of the dissected mucous membrane was included in a purse-string ligature, and the rectum thus occluded. Having in this manner provided against the accidental escape of the rectal contents during the operation, the ligature strings were used as a handle to pull down the gut, and exsection of the gut proper was begun and continued with careful precautions as regards the loss of blood, each mass of tissue being secured with a double ligature and division made between, so that there was not more than two ounces of blood lost. Five and a half inches of the gut were removed, including the diseased portion and fully one inch of the healthy mucous membrane beyond. The gut was then cut through and sewed to the edge of the incision in the integument with four stitches. Throughout the operation, the wound was irrigated frequently and thoroughly with a bichloride solution, 1 : 2500. Two drainage-tubes were introduced into the bottom of the wound, and the wound was then loosely filled with iodoform gauze. The whole was covered with sublimate gauze, and the outer dressing was removed as often as was necessary to secure cleanliness. The iodoform gauze was allowed to remain until it could be easily removed. There was no shock attending the operation. The temperature remained normal until the fifth day, when there was some intestinal disturbance due to constipation, showing that careful preparatory treatment did not suffice to clear out the bowels thoroughly. As soon as this was relieved by castor oil, the temperature fell to normal and the wound healed without interruption, and union was complete in about four weeks. Dr. Gerster said he proposed to perform a further operation with a view to changing the location of the anus, after which the patient would again be presented to the Society.

DR. POST asked if any of the members had seen malignant disease of the rectum in so young a patient.

DR. KEYES had had a case in a young woman upon whom he operated whose mother at the time of the operation was suffering from the same affection. The daughter was 22 years of age when she was operated upon.

DR. POST remarked that he had a brother who died of cancer of the rectum at the age of 29 years.

DR. GERSTER then read a paper entitled

THE SURGICAL DISSEMINATION OF CANCER.

(See page 230.)

DR. POST asked if, in the application of the Esmarch bandage upon the distal side of a malignant growth in an extremity, the pressure would not have a tendency to propel the cancerous corpuscles toward the trunk.

DR. GERSTER replied undoubtedly, and said that he spoke only of constriction of the limbs.

DR. SANDS asked Dr. Gerster whether, in the experiments he had made of injecting tumors prior to their removal with some coloring substance which was capable of being transmitted along the lymphatic vessels, he had obtained any results confirmatory of the theory he had advanced in his paper.

DR. GERSTER replied that he had obtained some results which, on superficial examination, might be declared to confirm his assumption; but from further and more careful study of the results of other experiments, the value of the former results was placed very much in doubt. In the first case, he used an emulsion of India ink, glycerine, and corrosive sublimate, 1 : 1000, which he injected into the tumor immediately preceding the operation. On examining the specimen, it became doubtful whether the injection had propelled the India-ink into the lymphatics, or whether this was done by manipulations incident to the operation. Another thing which raised doubt with reference to the value of the first experiment was the fact that corrosive sublimate arrests protoplasmic action. Dr. Gerster's object was to introduce this colored material so that the embryonic cancer cells, which have an amœboid movement, might absorb or take into them particles of India-ink, and then be carried onward into the tissues. If he had found such a cell far away from the place of injection, it would have been almost conclusive proof. Nothing of this kind, however, was found, because the corrosive sublimate paralyzed the action of the cells. Therefore, in the last two cases, he had used a warm solution of salt, 6 : 1000; and, as the specimens were still hardening, he was unable to report concerning the results.

The object of the paper was to direct attention to the clinical features of the subject, namely, that after operations where insufficient tissue was removed, relapses will be established, and the disease will reappear within four weeks after the operation. It would be most interesting to hear of the clinical experience of the surgeons present concerning the disproportionately rapid reappearance of malignant disease, as it had been mentioned, especially by the older writers. It is known that formerly operation was discouraged, because it was believed to induce more rapid recurrence and extension of the disease.

DR. BRIDDON had in mind three or four cases of benign tumors, one in particular, situated in the neck, and operated upon by the late Dr. Buck, and the operation was followed by a disseminated carcinosis of very rapid development. Another case occurred to himself,

in which a granular ulceration, probably epithelioma, of the index-finger was operated upon by amputation, and general carcinosis occurred within six weeks, involving the glandular system, and also the abdominal viscera.

His experience with regard to cancer had been that in those cases in which there was infiltration of the tissue immediately surrounding the tumor, or there were nodules of cancer in the vicinity of the tumor, rapid recurrence of the disease had followed the operation. He recalled the fact that several years ago, Mr. Arnot, of London, introduced the treatment of malignant growths by compression, and his recollection was that some good was accomplished by the method. At all events, he scarcely thought it increased the rapidity of the growth.

DR. POST referred to the fact that Dr. Batchelder, of this city, introduced the method of treatment by the use of compressed sponges, and in some cases, at least, with apparent good results.

DR. SANDS said that it had been his belief for a long time that in cases of malignant disease, especially of the breast, harm was often done by stimulating applications employed with the object of dispersing the tumor. He had always feared that such applications might prove injurious by exciting hyperæmia of the affected parts and thereby causing a more rapid increase of the swelling. It had not been his experience that operations performed for the relief of cancer had been followed by rapid recurrence, unless there was reason to suppose that the morbid growth had not been thoroughly removed. It seemed to him rather that the chance of exemption from recurrence was in a direct ratio to the completeness with which the work of extirpation could be accomplished. That seemed to be the only explanation of some cases in which what is termed permanent cure had been the result of the operation. He had listened with great interest to the remarks made by Dr. Gerster, who had raised an important question, which, perhaps, could be solved more easily by experiment than by clinical observation. As to the actual risk incurred at the time of operation, it would be hard to decide. He did not wish to disparage a theory which was both rational and plausible, but he thought it might be doubted whether, during an operation performed for the removal of a malignant tumor, the compression exerted would always exceed that to which it might have been subjected at various times prior to the operation. He could also recall instances in which the removal of the tumor had been effected only with very great difficulty, and with the exercise of considerable force, yet in which the patients had afterwards remained exempt from recurrence for a long period. He had presented to the Society within the past year a man about thirty years of age, upon whom he had operated fourteen years previously for fibro-sarcoma of the parotid, necessitating the removal of the entire gland, which was accomplished with great difficulty. Nevertheless the patient remained free from recurrence for more than thirteen years, when the disease reappeared in the cervical lymphatic glands.

DR. POST thought that incomplete removal of cancerous growths favored recurrence of the disease. Recently he had read an account of removal of a nasopharyngeal polypus piecemeal—that is, portions at different sittings—until finally the entire growth was removed; and the writer had stated that, during all this time, there was progressive improvement in the symptoms—a statement which struck Dr. Post as being contrary to the general experience of surgeons in dealing with malignant disease.

DR. SANDS remarked that the settlement of the point in question by clinical observation would be rendered difficult by the acknowledged diversity shown by different malignant growths regarding their tendency to dissemination.

DR. GERSTER said he fully admitted the fact that incomplete operations favored relapses. At the same time there were cases on record in which incomplete operations were said to have been followed by cure, although he had doubted such cases very much. He would like to ask why it was that this recurrent new growth changed its type? Why does the recurrent tumor behave entirely differently from the tumor first removed? It was a common experience, even in his short surgical experience, that a tumor which had been growing slowly, after it had been operated upon incompletely, grew much more rapidly than it had ever done. Of course, what he had said was only a suggestion of the necessity of continued investigation, caused by the desire of drawing attention to a fact hitherto little considered in our efforts to combat such an intractable disease as cancer.

DR. SANDS referred to one well-known circumstance which strongly favored the view taken by the writer of the paper—that is, the usually rapid extension of cancer of the tongue, which had been explained by the great mobility of the organ and its constant movements.

DR. MARKOE said that, on the other hand, it is to be observed that one of the most rapidly recurrent forms of malignant disease is that which occurs in the testicle, recurring generally within a very short period of time, and yet there is no operation for extirpation in which the tumor is less manipulated, and in which there is less displacement of cancer cells or juices than in extirpation of this organ.

DR. SANDS remarked that in such a case it would be difficult to decide whether dissemination had not already taken place at the time of the extirpation, because the parts in which secondary deposits occurred were so deeply situated along the posterior abdominal wall that until they had attained a considerable size they would not be recognizable. He had supposed that this was the reason why the removal of the testicle for malignant disease was so rarely followed by a permanent cure. Occasionally, however, sarcoma of the testicle was followed by exemption from recurrence. He had had one such case in which the disease had not returned thirteen years after operation.

DR. POST said there was one practice not uncommon, that of puncturing a tumor in cases of doubtful diagnosis as a means of ascertaining its character. It appeared to him that puncturing, if the disease were malignant, had a tendency to increase the malignant growth. He referred to a case in point during one of his terms of attendance at the New York Hospital, that of a tumor in the region of the scapula which had developed quite rapidly, and there was some question as to whether it was malignant or inflammatory. Dr. Post had reached the conclusion that it was malignant.

Dr. Mott thought it was inflammatory, and suggested that it be punctured for the purpose of ascertaining whether or not it contained pus. The puncture was made, and the operation was followed by great aggravation of the symptoms, and the patient died from the disease. From the time the puncture was made the disease progressed more rapidly than it had done at any time previously. He thought, therefore, if puncture was made it should be in cases in which extirpation could be immediately resorted to, if the diagnosis of malignant disease was confirmed.

DR. KEYES had had a case quite recently in which the patient also had been seen by Dr. Sands and by Dr. Markoe. It was that of a young man who was born with a mole on his back between the shoulders, which gave him no trouble whatever until he had reached the age of twenty-three or twenty-four years, when another little brown spot appeared within an inch of the original mole, and shortly after became excoriated, began to ooze a peculiar fluid, and looked angry. When Dr. Sands saw the patient, he advised removal of both dark spots, one of which had never ulcerated. The father of the patient had a mole in the same location, but it had never given him any trouble. There was no thickening about the brown spot, so that there was no considerable tumor. These tumors were removed in a neighboring city by the patient's surgeon, and within a reasonable number of months afterwards there appeared upon the back over the scapula upon the same side a lump, as large as a pigeon's egg, which grew rapidly, together with a tumor in the axilla of the size of a goose egg. These lumps were manipulated a good deal in the patient's native town, after which he came to New York where they were again subjected to a great deal of manipulation for purposes of diagnosis. Dr. Keyes supposed the tumor to be a recurrence of the original disease, and advised immediate removal. Dr. Janeway also saw the patient, as did also Dr. Markoe; both confirmed the diagnosis. The tumors were thoroughly removed at the patient's home. Dr. Janeway made the microscopical examination and pronounced both lumps to be melano-sarcoma. There were no other tumors about the patient's body; he recovered from the operation rapidly, and when he visited Dr. Keyes, in November last, he was apparently perfectly well. He promised that on the least appearance of any tumor at any part of the body he would report himself, and, inasmuch, as he had not returned nor reported, Dr. Keyes took it for granted that the disease had not recurred.

As to sarcoma of the testicle, Dr. Keyes had removed a number of them, mostly in hospital, and his experience had been that recurrence had taken place generally in the retro-peritoneal glands. In one case, however, that of a sea captain, there had not been any recurrence after a number of years.

Dr. Keyes further referred to another case in which he performed a double operation. The disease was situated in the parotid gland. A small nodule, which had been under observation for two or three years, was removed from the substance of the parotid. A salivary fistula followed, and a great deal of local disturbance ensued, but from all this the patient recovered. On examination microscopically, the growth was found to contain mucoid tissue, round-celled sarcomatous tissue, and certain alveoli packed with cells, which led to the opinion that the growth was undergoing cancerous degeneration, being wha is called a composite tumor of the parotid. The tumor was apparently enclosed in an outside shell, with budding portions, which were softer than the rest of the mass. There was no recurrence after removal for seven years, and then in the line of the old trouble a new nodule developed, which, within one year or eighteen months, was larger than the original growth—perhaps as large as the first phalanx of the thumb. Dr. Keyes removed the second nodule in the spring of 1883. It was an exceedingly disagreeable operation, because the patient became partially asphyxiated by mucus in the larynx, and it was necessary to perform tracheotomy in great haste to save life, and there was considerable hemorrhage, etc. He removed the tumor completely, and with it a small portion of the seventh nerve. The patient recovered promptly and completely; did not have a recurrence of the growth until about one month ago, when a small nodule developed under the angle of the jaw, about half an inch from the old cicatrix, rather painful and hard, not connected with the immediate site of the old operation. The second tumor was examined by Dr. Welsh, who decided that it contained mucoid tissue, sarcomatous tissue with alveoli packed with cells, and branching into the mucoid tissue. Probably the tumor now growing would prove to be a growth analogous to the others which had been removed from the same region.

DR. LANGE recalled one case in which he thought rapid spread of malignant disease was dependent upon surgical interference. The case occurred in the person of a young woman who was operated upon by Prof. Esmarch while Dr. Lange was his assistant, for removal of sarcoma of one of the ovaries. The tumor was so large that it was difficult to bring it outside of the abdomen, and it was necessary to crush it in order to remove it. In this attempt a certain amount of its contents got into the peritoneal cavity. Otherwise the operation was readily performed. The tumor was pedunculated and removed within entirely healthy parts, and there was also no other part of the sexual organs involved. The patient did well for the first two weeks, when nodules could be felt in different parts of the abdominal wall, and, as he recollected, it was not more than two months after the operation when the patient died from diffuse sarcoma of the peritoneum and sarcomatous degeneration of the entire diaphragm, and also of the diaphragmatic pleura. Dr. Lange had reported the case in 1877 as one which illustrated the spread of malignant disease from the original point, involving the peritoneum and other tissues.

CORRESPONDENCE.

THE SPRAY IN OVARIOTOMY.

To the Editor of THE MEDICAL NEWS.

SIR: The following is an extract from a recent work by Dr. Emmet (Emmet's *Principles and Practice of Gynæcology*, p. 715): "In this country, I do not know of any prominent operator who now employs the carbolic acid spray." This statement implies that the writer is not persuaded of the value of spray in ovariotomy. My own experience has led me to an opposite

opinion. Indeed, I should not like to do a laparotomy for any purpose without antiseptic spray. I have been led to this conclusion by the results of 183 cases of removal of cystic ovaries, of which I have lost only 21; but more especially by the result of the last 100 of these cases, only 10 of which were fatal, while 38 were consecutively successful. I feel that to omit the antiseptic spray would be to deprive the patient of one of the ready and efficient elements of success. As I can hardly hope for much better results than those I have cited, and being quite content to let well alone, I shall hesitate before disturbing my present plan of operation by giving up a detail to which I attach much importance.

Very respectfully, your obedient servant,

JOHN HOMANS, M.D.

161 BEACON STREET, BOSTON, Feb. 17, 1885.

LAPAROTOMY FOR GUNSHOT WOUND OF THE SMALL INTESTINE.

To the Editor of THE MEDICAL NEWS.

SIR: In the editorial on "Laparotomy for Gunshot Wound of the Small Intestine," contained in the number for February 21st, you express an opinion in favor of uniting the ends of resected intestine and adverse to the formation of an artificial anus. As the opinion is positive and without qualification, allow me to call attention to the fact that there is not an unanimity upon the point. For instance, in a note to the report of a case of excision of the cæcum for epithelioma, by Mr. Whitehead, surgeon to the Manchester Infirmary, published in the *British Medical Journal*, of January 24th, it is stated that Ruchel has collected 121 cases of resection of the intestine "the conclusion being that the two ends of the bowel should not be united at the time of resection, but that an artificial anus should be established." My impression is that this is the teaching of Mr. Bryant in his excellent series of papers on intestinal obstruction, published in the same journal towards the close of last year, but I have not access to them at this moment. In an operation of this magnitude, now rapidly coming before the profession as a duty, it is desirable that there should be clear understanding of its steps in detail. May I not request you, therefore, to present, at an early date, the evidence and the teaching upon this most important point in progressive surgery?

Yours truly,

J. C. R.

DAYTON, OHIO.

OBITUARY.

LOUIS ELSBERG, M.D.

DR. LOUIS ELSBERG, of New York City, Professor of Laryngology at Dartmouth Medical College, Professor of Laryngology and Rhinology at the New York Polyclinic, and Physician to the Throat Wards in Charity Hospital, New York, died at his residence in New York on the 19th of this month, by pulmonary œdema after a ten days' illness, following a cold contracted in returning from a lecture he had just given at the Polyclinic. For two years he had been suffering with Bright's disease, and although of late his health had become sufficiently reëstablished to relieve his professional advisers from all concern as to any immediate danger his condition was such that he was unable to rally. He was in the 48th year of his life, having been born at Iserlohn, Prussia, April 7, 1837. He leaves a widow and a daughter.

Dr. Elsberg had been prominently before the profession for more than a quarter of a century as having introduced laryngoscopy into America, and as having developed the art successfully as a diagnostician and as a practitioner. His reputation was world-wide and was early established, chiefly by an "Essay on Morbid Growths of the Larynx," which won the prize of the American Medical Association in 1864.

His family came to reside in Philadelphia about 1849, and young Elsberg entered the public schools, graduating in 1854 from the Central High School with the highest honor of his class, and delivering the honorary address at the Commencement. Entering Jefferson Medical College a year later, he graduated from that institution in 1857. He then received the appointment of Resident Physician in what is now the Mount Sinai Hospital of New York, and at the expiration of his term of service, made a visit to the medical centres of Europe. He reached Germany at the time that Czermak was bringing laryngoscopy into notice, and was a member of the first class taught by him, his classmates being Prof. Stœrck, of Vienna, Prof. Lewin, of Berlin, and Dr. Semeleder, subsequently Surgeon to the Emperor Maximilian, whom he accompanied to Mexico, where he remained. Semeleder published the first volume on *Rhinoscopy*, but became interested in other branches of medicine, and is comparatively unknown as a laryngologist; but the three others remained continuously devoted to laryngology, each one of them acquiring professional renown in connection therewith.

In 1861, Dr. Elsberg was made Professor of Laryngology and Throat Diseases in the Medical Department of the University of New York, and established there the first throat clinic instituted anywhere. His connection with the University was severed in 1878. He founded the New York Laryngological Society, and presided over it for many consecutive years. He was the first President of the American Laryngological Association, and was reëlected at the close of his term. He founded the *Archives of Laryngology* in 1880.

Dr. Elsberg was an enthusiast in his profession, and taught his specialty by precept, by example, and by many valuable contributions to current medical literature. While this brought him well-earned fame, his large practice seems to have afforded him no leisure for writing a systematic treatise on his specialty, such as had been long expected from him and as would have had an extensive circulation;—a loss to American medical literature now irreparable. His numerous papers scattered in medical journals and Society Transactions evince independent thought, original research, and comprehensiveness of design. While his reputation rests chiefly on his laryngological work, it is enhanced by original investigations in histology and in acoustics, which are less known to medical men generally than to those interested in these branches.

Dr. Elsberg was a close student, an omnivorous reader, and an admirer of systematic classifications. He was an affectionate relative, a staunch friend, and

a sympathetic physician. His death is a serious loss to American medicine.

AT a meeting of the members of the medical profession in attendance at the New York Polyclinic, held on February 21, 1885, the following resolutions were unanimously adopted:

Whereas, In the inscrutable Providence of God, PROFESSOR LOUIS ELSBERG has been removed from this life.

Resolved, That in his death science has lost an earnest and indefatigable student, the medical profession an honorable member, and society one with broad and humanitarian sympathies.

Resolved, That we offer to his family assurances of sympathy, and as a testimony of our regard for his character and attainments and a sense of our personal loss in his death, that we, the members of the class of the New York Polyclinic, attend the funeral in a body.

Resolved, That copies of these resolutions be sent to his family, to the Faculty of the Polyclinic, and to the medical press.

NEWS ITEMS.

MEETING OF THE SANITARY COUNCIL OF THE MISSISSIPPI VALLEY.—The Executive Committee of the Sanitary Council of the Mississippi Valley has fixed the date of the seventh annual meeting of the Council for Tuesday, March 10th prox., and in the city of New Orleans. This is about a month earlier than its meetings are usually held, and the committee assigns as a reason for the change the probability of Asiatic cholera reappearing in the country, and the uncertainty concerning National legislation on public health matters. Invitations are extended to all State and local health authorities in the Valley, and to representatives of commercial and transportation interests.

ELECTION OF FOREIGN MEMBERS OF THE SOCIÉTÉ DE CHIRURGIE.—At a meeting of the Société de Chirurgie de Paris, held Jan. 21, 1885, MM. Pellizzari, of Florence, and Sklifossowski, of Moscow, were elected foreign associates; Drs. Lewis A. Stimson, of New York, Saltzmann, of Helsingfors, Plum and Studsgaard, of Copenhagen, foreign correspondents.

THE SOCIÉTÉ MÉDICO-PRATIQUE DE PARIS has elected Prof. Roberts Bartholow, M.D., of Philadelphia, a corresponding member.

HARVEY'S MANUSCRIPT LECTURES.—The MS. of the original lectures at the Royal College of Physicians by William Harvey, including his earliest observations on the heart and circulation, and delivered by him in and after 1616, were rediscovered in the British Museum in 1877. Dr. E. H. Sieveking gave a description of the little book, and exhibited an autotype copy of one page in his Harveian oration at the College in 1877. He then suggested that it would redound to the honor of the present generation, and be an advantage to the history of medicine, if the whole of the lectures could be published in autotype, accompanied by an intelligible transcript. The handwriting is so crabbed, and there are so many abbreviations, that no one but an expert could succeed in understanding them. Without the valuable aid of Mr. Bond, now the Chief of the British Museum, Dr. Sieveking would have failed in his attempts to understand much, if anything, of the lectures. By dint of severe labor, Mr. Bond succeeded in interpreting one of the lectures, and has now been good enough to make Dr. Sieveking acquainted with a gentleman who will undertake to transcribe the whole of the lectures. Dr. Sieveking's inquiries lead him to believe that no publisher could be found to undertake the risk of publication in the form proposed, unless guaranteed a certain amount of professional support. On the other hand, he calculates that if from two to three hundred gentlemen would engage each to take a copy at a price not exceeding two guineas, the work might be safely proceeded with. Autotyping is a much more expensive process than ordinary printing, and the honorarium to the transcriber would necessarily add considerably to the cost.

Dr. Sieveking, in a letter to the *Lancet*, asks his professional brethren whether they will aid in this labor of love of, and admiration for, our great prototype of the scientific physician? He states that the Presidents of the Royal Colleges of Physicians and Surgeons warmly support the undertaking.

Any communications on the subject may be addressed to Edward H. Sieveking, M.D., 17 Manchester Square, W., or to Messrs. Churchill, 11 New Burlington Street, W., London.

OFFICIAL LIST OF CHANGES IN THE STATIONS AND DUTIES OF OFFICERS SERVING IN THE MEDICAL DEPARTMENT, U. S. ARMY, FROM FEBRUARY 17 TO FEBRUARY 24, 1885.

BENTLEY, EDWIN, *Major and Surgeon.*—Relieved from further duty at Fort Clark, Texas, and assigned to duty as Post Surgeon, Fort Brown, Texas.—*S. O. 17, Department of Texas*, Feb. 16, 1885.

PATYKI, JULIUS H., *Captain and Assistant Surgeon.*—Leave of absence further extended seven months, on surgeon's certificate of disability.—*S. O. 40, A G. O.*, Feb. 17, 1885.

TAYLOR, M. E., *Captain and Assistant Surgeon.*—Assigned to duty at Fort Stanton, New Mexico, as Post Surgeon.—*S. O. 29, Department of the Missouri*, Feb. 21, 1885.

PERLEY, H. O., *Captain and Assistant Surgeon.*—Granted leave of absence for one month, to take effect about March 5, 1885.—*S. O. 16, Department of Dakota*, Feb. 10, 1885.

ROBINSON, S. Q., *Captain and Assistant Surgeon.*—Relieved from duty at Fort Spokane, Wyoming Territory, and ordered for duty as Post Surgeon, Fort Klamath, Oregon.—*S. O. 23, Department of Colorado*, Feb. 9, 1885.

OFFICIAL LIST OF CHANGES OF STATIONS AND DUTIES OF MEDICAL OFFICERS OF THE UNITED STATES MARINE-HOSPITAL SERVICE, FOR THE WEEK ENDING FEBRUARY 21, 1885.

BATTLE, K. P., *Assistant Surgeon.*—To proceed to Pittsburg, Pa., for temporary duty, Feb. 19, 1885.

RESIGNATION.

HEATH, W. H., *Passed Assistant Surgeon.*—Resignation accepted, as tendered, by the Secretary of the Treasury, Feb. 14, 1885.

PROMOTION.

KALLOCH, P. C., *Assistant Surgeon.*—Promoted and appointed Passed Assistant Surgeon by the Secretary of the Treasury, from March 1, 1885, Feb. 19, 1885.